A TRADE UNION ANALYSIS OF TIME STUDY

SECOND EDITION

WILLIAM GOMBERG

Prentice-Hall
Industrial Relations and Personnel Series

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PRENTICE-HALL INDUSTRIAL RELATIONS AND PERSONNEL SERIES

Dale Yoder, Editor

A Trade Union Analysis of TIME STUDY

by

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With a Foreword by

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Second Edition

New York
PRENTICE-HALL, INC.
1955

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by

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70 Fifth Avenue
New York

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L.C. Cat. Card No.: 55-7251

PRINTED IN THE UNITED STATES OF AMERICA

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7-10-61 Publ Bus Ad

Foreword

My endorsement of Dr. Gomberg's book, devoted to the exposition of time study techniques as a basic method for compensating labor, is perforce circumscribed by a layman's outlook on this subject. Not being an engineer, I shall not venture to appraise its contents except from the vantage point of a labor leader who has watched the application of time study methods in his industry.

I should like to say, first, that for many years labor unions had looked askance at time study techniques. Trade unionists had been prone to regard these studies and techniques as management tools for labor speed-ups and for dilution of skills in industries where skills are valued factors. The fact that management in the past sought to exclude labor from taking part or exercising any control over time studies served to strengthen this prejudice.

A revision of this viewpoint in the women's garment industry, which is chiefly a piece-rate industry, began in the early thirties when the sweep of collective bargaining compelled the adoption of new approaches to piece-rate price settlement in one of our largest markets, the New York dress industry. The adoption of the "unit system" proposed by our union leadership for price settlements on dresses was, in reality, the first union-initiated step toward industrial engineering in the manufacture of women's garments. This move coincided with the employment by several large garment firms, located chiefly in markets out-

side of New York, of industrial engineers who were charged with the task of installing "scientific" piece rates in their plants.

We realized, of course, that the employers who had gone in for industrial engineering were chiefly motivated by prospects of rationalizing costs. Still, having already had some experience jointly with our New York dress employers in "unit system" price settlements, we could not fail in discerning in these new engineering efforts certain definite advantages to labor, provided the employment of these time study techniques was not the sole business of management. We saw that labor must be afforded participation on an equal basis as well as an opportunity to share in the benefits of these techniques in order to protect the interests of the worker and his earnings.

We followed this idea through by establishing a Management Engineering Department in 1941, the direction of which we entrusted to William Gomberg. We are satisfied that this department has since its inception done highly useful work by solving many knotty collective bargaining problems in scores of plants, problems in which both the owners and the workers were gravely concerned.

May I add that the choice of Dr. Gomberg as head of our Management Engineering Department has been a happy one. His entire adult life, since graduation from college, has been identified with union service in various capacities. I trust that this book, dedicated to the analysis of time studies as a collective bargaining tool, will find wide acceptance among the ever-increasing number of industry leaders and those in the industrial engineering profession who are genuinely concerned with balanced labor-management relations.

DAVID DUBINSKY

Introduction to Second Edition

When *A Trade Union Analysis of Time Study* first appeared, the concepts presented were examined on an a priori basis. A statistical model was projected and an experimental outline implied to verify the thesis stated. Adam Abruzzi has taken up the study where *A Trade Union Analysis of Time Study* ended, and his *Work Measurement* demonstrates the validity of the original thesis on an a posteriori basis. The two works complement one another.

It is now eight years since the first edition of *A Trade Union Analysis of Time Study* appeared. Statistical methods of treating time study data had received no application up to that time. Any attempt to think of the time study problem as a statistical problem of sampling was contemptuously dismissed as numerical nonsense. Labor's right to participate in the process of rate setting remained to be finally established.

Since the publication of the original edition of the manuscript, many new scholars have appeared to defend and implement the application of statistical methods to time study. Some of the suggested hypotheses of *A Trade Union Analysis of Time Study,* more notably those on standard data, were pursued and verified with experimental evidence by Adam Abruzzi, Gerald Nadler, and others. Other scholars, like Harold O. Davidson in *Functions and Bases of Time Standards,* suggested alternative approaches to the original time study, but within a statistical frame of reference.

Marvin Mundel and many of his students, notably Robert N. Lehrer,[1] suggested the application of statistical techniques to different phases of the time study process, though Mundel still insisted that time studies must be rated. He has attempted to promulgate a new approach to rating called objective rating. Dr. Lehrer has proposed a standard work measurement control system dependent upon a Shewhart model very similar to the writer's.

Finally, the report of the Society for the Advancement of Management rating project itself is couched in statistical terms. Although many of us may take issue with its conclusions and methodology, it is nevertheless significant that the authors of the report were careful to point out that there were alternative methods of treating the data they collated statistically. This in itself is tribute enough to the new pervasive influence of statistical methodology in time study.

Meanwhile, the arbitration decision settling the strike of the Ford Motor Company workers against an alleged speed-up in 1949 spelled out the limited nature of management's right to set production standards, even where this function was made a unilateral right of management in the collective bargaining contract.

When the management division of the American Society of Mechanical Engineers reviewed the progress of the scientific management movement between 1942 and 1952, they wrote that the critical evaluation of work measurement techniques reached a high during this period and stated that this critical movement reached its highest point with the publication of *A Trade Union Analysis of Time Study*.[2]

It is our hope in this revised edition to re-examine the field; we will review the new techniques and approaches which have been added to the field and ask: Where do we go from here?

Well, why a new edition at this time? Is the time study problem any less acute economically and socially than it was in

[1] "Statistical Work Measurement Control," *Advanced Management*, Vol. XVII, No. 8, August 1952, pp. 10–11.

[2] *Transaction of the A.S.M.E.*, Vol. 75, No. 4, May 1953, p. 669.

1948? Are we any closer to the resolution of disputes that arise between worker and employer over production standards? The June 21, 1953, issue of *The New York Times* states the following. It describes the beginning of the uprising of the workers in East Berlin: "Trouble began innocently enough. Tuesday morning [June 16, 1953] hundreds of workers assembled outside Stalin Allee to protest a recent increase in work quotas. . . . Again and again, slogans of revolt rose from the masses: We don't need a norm, we're in form."[3] By the next day all of East Germany was blazing in revolt. It recalled a book written thousands of years earlier: "And Pharaoh commanded the same day the taskmasters of the people, and their officers, saying, 'Ye shall no more give the people straw to make brick, as heretofore: let them go and gather straw for themselves. And the tale of the bricks, which they did make heretofore, ye shall lay upon them; ye shall not diminish ought thereof: for they be idle; let there more work be laid upon the men, that they may labour therein; and let them not regard vain words."[4] The results of the conflict have reverberated through the ages. It was the beginning of the revolution creating the Judaic-Christian ethic.

It is unlikely that anything written by myself or my colleagues will solve this problem for all time. All that we can do is keep up with new developments in the field within this moral tradition.

[3] *Justice,* Vol. XXXV, No. 13, July 1, 1953.

[4] Holy Scripture, Exodus 5:7, 8 (Philadelphia: Jewish Publication Society, 1953).

Introduction to First Edition

The material for this book has been gathered in the course of my professional work, performed as Director of the Management Engineering Department of the International Ladies' Garment Workers' Union and as consultant to a variety of other unions, affiliated with both the American Federation of Labor and the Congress of Industrial Organizations, in the solution of problems involving the setting of production standards.

My primary purpose in writing this book is to reach the active time study practitioner. The profession abounds with "cook books" on how to take a time study. These manuals set out a procedure to be followed by a time study observer in setting up a study, reading the observations, and finally analyzing the data for a conclusive production standard.

These manuals go under a variety of titles, but in none is an attempt made to examine the basic assumptions at the foundation of the procedures. There has been a paucity of literature analyzing the rational basis behind time study. With the exception of some small pamphlets published by the British Industrial Health Board[1] and the American Management Association,[2] the only attempt to analyze time study critically has been Ralph Presgrave's *Dynamics of Time Study*.[3]

[1] E. Farmer, *Time and Motion Study*, Industrial Fatigue Research Board, Report No. 14 (London: His Majesty's Stationery Office, 1923).

[2] Richard Stephen Uhrbrock, *A Psychologist Looks At Wage Incentives* (New York: American Management Association, 1935).

[3] Ralph Presgrave, *The Dynamics of Time Study* (2d ed., New York: McGraw-Hill Book Co., 1945).

I do not pretend to have made an exhaustive analysis of all American industrial time study practice. Even if a statistically sound sample of all firms using every type of time study practice could be developed, in all likelihood the controversy surrounding the technique would make the business concerns loath to part with their business secrets, their production standards.

The standard texts in the field are generally recognized as the authoritative statements on time study and the detailed procedures they recommend find widespread application in American industry.

The factory manuals I have examined conform in most practical aspects, with few exceptions, to the recommendations in the standard texts.

I have taken a different approach from these texts as will soon become apparent to the reader. It is my hope that through professional self-analysis members of the profession will produce time study manuals that possess firmer foundations than do those that appear today.

When you examine the validity of existing industrial time study practice, you soon find that you are in a field filled with conflicting methods of observation, conflicting methods of collating data, conflicting methods of analyzing and interpreting data, and, finally, conflicting results. The results are generally accompanied by an insistence that these are scientific results in accordance with the facts.

The word "scientific" is bandied about so loosely in this amorphous field that any investigator may well begin, as I have done, with a re-examination of the nature of the scientific method and the kind of problem posed by time study within the requirements set by the scientific method.

From there I have examined contemporary time study techniques to determine how closely existing time study practice conforms to the requirements of a scientific technique.

This investigation then leads to an examination of fundamental problems in the fields of measurement, probability, psychology, physiology, sociology, and machine characteristics.

This is only natural, inasmuch as time study stands at the bridgehead of the social and physical sciences, involving the very intimate relationship between man and the tools which he uses.

Finally, I examine the possibilities of the application of this scientific system to practical everyday rate problems.

The book is in two parts:

Part One examines the theoretical requirements for a science of time study, if such a thing is possible.

Part Two examines how closely existing industrial time study practice comes to the scientific ideal.

Acknowledgments

Acknowledging my indebtedness for the material in this volume is exceedingly difficult. It is the result of many conversations with members of the engineering profession and with trade union associates. To all of them, those specifically mentioned in the book and the many others whose help has been invaluable, appreciation is given.

Special acknowledgment should be reserved for the members and leaders of the International Ladies' Garment Workers' Union. This Union has become over four decades a great collective bargaining laboratory which has pioneered many of the tools that are today accepted as commonplace in the labor movement. This book is presented in the hope that it satisfies that tradition.

Special thanks are due, moreover, to Professor Walter Rautenstrauch of the Industrial Engineering Faculty of Columbia University, who guided much of the investigation and served as a patient sounding board for many an idea before it reached finality; and to the members of the staff of the Management Engineering Department of the International Ladies' Garment Workers' Union, who collated a good deal of the material upon which the book is based. To Hy Fish, Millie Glazer, my former secretary, Theresa Falaguerra, my present secretary who is responsible for many parts of the revision, I am deeply indebted for invaluable help.

Acknowledgment is also made to the following publishers

who have generously given permission to quote from their copyrighted works as indicated in the text:
American Institute of Industrial Engineers; Appleton-Century-Crofts, Inc.; Chemical Publishing Co.; Columbia University Press; Harcourt, Brace and Company; Harper and Brothers; Harvard University Press; Harvard University Graduate School of Business Administration; Henry Holt and Company; Her Majesty's Stationery Office, Research Council Publications' Office; Industrial Relations Research Association; International Confederation of Free Trade Unions; International Labor Office; Alfred A. Knopf Incorporated; Little, Brown & Company; Longmans, Green and Co. Ltd.; Macmillan Company; McGraw-Hill Book Company; National Planning Association; Prentice-Hall, Inc.; Regents of University of California; Ronald Press Company; Sawell Publications, Ltd.; School of Industrial and Labor Relations, Cornell University; Society for Advancement of Management; U. S. Department of Agriculture Graduate School; U. S. Department of Labor; University of Chicago Press; D. Van Nostrand Company; John Wiley & Sons; Williams & Wilkins Company; World Book Company; *Advanced Management; Factory Management and Maintenance; Harvard Business Review; Industrial Management Society; Journal of Aviation Medicine; Journal of Consulting Psychology; Magazines of Industry; Mechanical Engineering; Modern Management; Psychosomatic Medicine; Science; The New York Times;* The American Federation of Labor; The United Auto Workers (CIO); to McClure, Hadden and Ortman, Inc.; Lawrence Cohen; Harold O. Davidson; Philip J. Kinsler, Research Statistician; Oscar Mayer & Co.; R. B. Leng; L. C. Pigage, and I. L. Reis.

WILLIAM GOMBERG

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PART ONE

CHAPTER 1

Parallel Development

Industrial Engineering and Collective Bargaining through World War II

The industrial engineer works at the bridgehead where technological problems merge into social questions. The civil engineer, the electrical engineer, the mechanical engineer have always been applied economists. Their job has been to build a dam, design a dynamo, or fabricate metal shapes to yield a maximum output at a minimum cost.

The industrial engineer's techniques go beyond the mechanical cost factor. He is a designer of organization structures and administrative techniques to achieve specific industrial purposes and is therefore saddled with all of the problems attending human relationships. This means that the effective industrial engineer must become apt in the fields of anthropology, sociology, and psychology, among others. Above all, in a democratic society he must understand the relationship between efficiency and consent.

Many of the techniques which the engineer wants to use are frustrated by his inability to secure this consent from the working group. The labor movement today is the principal organized force through which the engineer speaks to and negotiates with workers. For example, the industrial engineer wants to raise productivity. Who can be against increased productivity? It's

like being against the ten commandments. He asks the trade unionist, "You're in favor of increased production, aren't you?" Then the industrial engineer goes to work. He pulls out a stop watch and is all but physically thrown out of the plant. He proposes a job evaluation system and is laughed at. He demonstrates with movies, if you please, that it is easier to move a piece of metal ten inches than ten feet, and still the worker insists upon his old ten-foot method.

Yes, the other engineers may have their problems, but nothing like these. If the civil engineer wants to understand what it's like to be an industrial engineer, let him visualize a dark, eerie Halloween night on which spirits, animate and inanimate, are abroad. Along comes the bridge, which is his pride and joy, spanning a majestic river and it addresses him in these accents, "Hey jerk, do you know that I could have remained standing and carried just as big a load if you used one-quarter the tonnage of steel that my poor piles must hold up?"

This is just an everyday experience for the industrial engineer. The appearance of the trade union in the picture merely gives open, organized expression to this feeling. It at least means that the engineer can deal with this problem through an organized group.

It is, of course, important that the engineer know what the attitude of the labor movement is toward any of the techniques he wants to install, but it is even more important that he understand the reasoning behind the union's attitude. The logic behind the labor movement's attitude toward time study, job evaluation, wage incentive payment plans, etc., is best understood against an examination of the nature of the trade union institution itself. How does the union think? How does its system of values, from which much of its thinking stems, differ from the customary reasoning of management?

An attempt will be made to examine the basic thinking behind the trade union, the development and evolution of its thinking, and the parallel histories of the scientific management movement and the trade unions as they interacted on each

other. We will then proceed to an analysis of the growth of a trade union philosophy of time study engineering.

Sidney and Beatrice Webb are the recognized historians of western trade unionism here and abroad. They trace the pressure for trade unionism to the divorce of the worker from the tools which he requires to do his work. Unions arose, they tell us, when

> . . . the great bulk of the workers had ceased to be independent producers, themselves controlling the processes and owning the materials and product of their labor, and had passed into the condition of lifelong wage earners, possessing neither the instruments of production nor the commodity in its finished state.[1]

Contrary to most general notions, trade unionism did not arise as a great revolt of a miserably paid unskilled proletariat. Philip Taft indicates that, "It was not the worker who had the lowest bargaining power but the one with the greatest sense of independence who pioneered the trade union movement."[2]

This was inevitable, for only the worker with the greatest sense of independence was willing to challenge the authority of the employer in the early days of organization, and it required some threat to existing customs and standards to initiate organization. Therefore, it was not the propertyless proletariat of Marx, but the labor aristocrat who was the pioneer of trade unionism.

This concept of the initial organization of trade unions is of great importance in understanding current labor movement behavior. It provides the rock upon which so many ideological pretensions, served up by both friendly intellectuals and ambitious labor leaders, have foundered.

Defining the objectives of the labor movement has been undertaken by social scientists, ideologists, and the labor leaders themselves. To begin with, the term labor movement is a much

[1] Sidney and Beatrice Webb, *The History of Trade Unions* (London: Printed by the authors for the students of the Workers Educational Association, 1919), p. 26.

[2] P. Taft, "Theory of the Labor Movement," *Interpreting the Labor Movement* (Industrial Relations Research Association, December, 1952), p. 5.

broader classification than trade union. Trade union activities themselves are generally confined to those which bear immediately upon questions of wages, hours, working conditions, and the direct pressure techniques, such as strikes, to secure these objectives. Labor movement activities include a much broader area, extending to such matters as politics, civic activities, and problems relating to the general welfare. The priority which any of these activities takes is not always reflected in the correlative amount of publicity which each of these activities receives. The general public is, therefore, likely to be confused about the central purpose of the trade union. This confusion was compounded by revolutionary ideologists who had hoped to use the trade unions as levers to overthrow existing social relationships and remake society into a new utopian image.

The fact of the matter is that the first classification of unions undertaken by Hoxie attempted to list them in accordance with the ideological pattern professed by their different current leaders.[3]

Hoxie's successors have revised his original listings somewhat, but to this day the rather vague term of conservative unionism *vs.* progressive unionism remains in common usage.

Conservative unions are loosely identified as business unions professing no particular long run objectives. They are supposed to be opposed to the use of the strike weapon, to emphasize craft jurisdictions, and continually to be embroiled in jurisdictional disputes. They are supposed to eschew political considerations and confine their solidarity and interest to their own immediate membership.

Progressive unions are likely to profess political attitudes varying from an enthusiasm for specific New Deal measures to a complete reorganization of society into a democratic co-operative commonwealth. They tend to emphasize the industrial form of organization. Their emphasis on working class solidarity is supposed to keep them clear of all jurisdictional contests.

[3] R. F. Hoxie, *Scientific Management and Labor* (New York: D. Appleton & Co., 1915).

Yet when we analyze the day to day activities of both of these types of unions, we are struck by their similarities rather than by their differences. Strangely enough both those unions which started with radical or socialistic aspirations and those which either did not question the social order or even expressed positive loyalty to private enterprise found that they were doing essentially the same thing. The philosophy pursued by unions in their actions, irrespective of any ideological professions, was first formulated by Professor Selig Perlman of the University of Wisconsin, whose theory of job-conscious unionism built the whole dynamics of the labor movement around the job.

Professor Perlman has pointed out that unions base themselves on a consciousness of limited job opportunities—a situation which requires that the individual worker, both in his own interest and in the interest of the group to which he belongs, occupy his job on the condition of observing the common rules formulated for the group by his union. The union in turn seeks, by collective bargaining with the employer, to establish rights in the jobs for both the individual and the whole group.[4]

The bankruptcy of these old ideological divisions becomes apparent when one examines the actual trade unions. An old conservative trade union like the A. F. of L. Teamsters Union has extended its jurisdiction to warehouses so that to all intents and purposes it has become an industrial union.

On the other hand, the C. I. O. United Auto Workers Union has been compelled to modify its industrial structure to give recognition to craft problems. For example, it has created a special tool and die council charged with taking up the problems of highly skilled workers.

The preoccupation of trade union leadership with the day to day problems of unions, irrespective of the political and ideological conviction professed by any of them, soon places any utopian panaceas in the background. Negotiating contracts, administering agreements, and handling grievances become the

[4] S. Perlman, *A Theory of the Labor Movement* (New York: Augustus M. Kelley, 1949).

full time job of the trade unionist. Political activity in which trade unionists do engage is strictly subordinated to these considerations.

This preoccupation has been the bane of many an ambitious intellectual. When a union is attempting to establish itself in its early developmental stages, it attracts a wide variety of people. The great majority are workers primarily interested in improving their working conditions. Some of these are dedicated idealists who look upon the trade union movement as a lever which will enable them to reconstruct society along models which they have developed out of either a Marxist or some other revolutionary philosophy. Others who are attracted are either conscious or unconscious agents of the Soviet foreign office who, as members of the Communist Party, seek influence in the organization to shape it to the needs of the Soviet foreign office. These needs may vary from very close collaboration with employers to raise production to pointless strikes designed to wreck the economy. A communist led union is not a left wing union. It is an American institution captured by the agents of a foreign power to serve the needs of that foreign power in very much the same manner that the prewar managements of some foreign corporations were institutions designed to serve the needs of the Nazi foreign office.

Sooner or later a conflict develops between the elements in the union who are attempting to serve the economic needs of the workers and those people who are dedicated to the use of the organization to promote the needs of their new model society. Very often the very leadership who may have come to power because they were driven by the idealistic vision of the new society they were going to create find that they are the victims of a new schizophrenia. They must choose between the immediate economic needs of their members and the long term requirements of their vision. The United States democratic atmosphere, which has always been pragmatically anti-ideological, leads them, if they are to remain as trade union leaders, to the satisfaction of short run economic ends.

The history of the last few years is replete with examples of progressive trade unionists who began as heroes of the *Daily Worker* only to be denounced later as arch villains. Other union officers have been pilloried by anti-Stalinist intellectuals for not making their trade union the springboard of power policies which would reshape society nearer to their heart's desire.

The manner in which a wedge is driven between the trade unionists and the ideologists is perhaps best illustrated by two examples. Some trade unionists had made it a habit to endorse some of the many communist "fronts" like the communist controlled Civil Rights Congress. Attacks upon them were ignored by these trade unionists. They recognized many of the people who had attacked them as people whom they had bitterly fought in order to organize their union in the first place. Sending delegates to a Civil Rights Congress didn't particularly interest them and didn't interfere with any critical economic problems which they had to face. On the other hand, when the Communist Party attempted to mobilize them to call irresponsible strikes or to protest the Marshall Plan, many of those unionists who had been pliable communist allies before broke with the Communist Party. Calling a noneconomic strike merely to contribute to chaos in the United States might serve the purposes of the Communist Party, but it would wreck the trade union which the leader heads. Denouncing the Marshall Plan might again serve the needs of the Soviet foreign office, but the inauguration of the Marshall Plan meant employment for many trade unionists in export industries. Confronted with this dilemma the trade unionist who was either friendly or indifferent to the communists had to become actively hostile.

The Socialist Party encountered a very similar experience, though for an altogether different set of reasons. Building a socialist force in the United States called for the election of socialist candidates. On the other hand, preserving the collective bargaining position of American unions called for the election of New Dealers like Herbert H. Lehman. Again, in the choice between ideological allegiance and the collective bargaining

needs of the union, the collective bargaining needs took precedence.

Both the Marxist and the classical economist find little patience with everyday undramatic trade unionism. The classical economist finds the union interference with the normal competitive forces governing wages and hours an unbearable assault upon his carefully nurtured escape from reality called a picture of our economy. The Marxist finds the trade union a barrier to his leadership of the working masses whom history has predestined as the vehicle to carry him to political power. The trade unionist in turn exclaims, "a pox on both their houses" and continues to pursue his aims. They are:

1. To provide a means for the democratic settling of wage rates and working conditions.
2. To replace the little oriental despotisms that formerly made up our business world with democracies governed by a code of juridical law.

Perhaps Tannenbaum has put it best when he says, "The trade union movement has survived because it satisfies the workers' craving for moral status in a recognizable society."[5]

The most important conclusion that an industrial engineer can draw from this picture of the labor movement is that the reaction of any particular organization to a proposal for an engineering installation will depend much more on the economic and industrial problems as they reveal themselves in collective bargaining than on either the ideological professions of the union leadership or the resolutions that are extracted from convention proceedings. For example, the constitution of the International Association of Machinists, A. F. of L., prohibits the recognition or promotion of incentive wage payment plans, yet collective bargaining realities have compelled the leadership of the I. A. M. to maintain staff specialists in these fields to cope with these problems as they arise. This is a good example of the

[5] Frank Tannenbaum, *A Philosophy of Labor* (New York: Alfred A. Knopf, Inc., 1951), p. 13.

cultural lag between the ceremonial expression of the labor movement and its actual operating behavior, dictated by common sense pragmatism.

Present practices can best be understood, however, against the background of the concurrent development of the American labor movement and the scientific management movement. The evolution of the trade union movement's attitude toward productivity and the impact of the collective bargaining process on productivity itself reveal a uniquely American phenomenon which has made the United States the most productive country in the world.

The publication of *A Differential Piece Rate* by Frederick Taylor in 1895 can be taken as the starting point of the scientific management movement. This was just nine years after Samuel Gompers had called the founding convention of the American Federation of Labor.

McKelvey has divided the history of the relationship between the scientific management movement and the labor movement into three distinct periods to which I have added a fourth.

1. A period of unmitigated hostility between scientific management and organized labor lasting from 1911 to 1915.
2. A period of transition extending from 1915 to 1917.
3. A period of friendliness and understanding between the two until the great depression.[6]
4. The emergence of a positive trade union philosophy of industrial engineering following the great organization drives and with the passage of the New Deal legislation.

The remainder of this section will treat the development of these first four periods.

Taylor's philosophy of scientific management left no room in the scheme of things for trade unionism. Actually, underpinning Taylor's philosophy was a doctrine of rugged individualism which left no room for any teamwork concept. Every worker

[6] J. T. McKelvey, *AFL Attitudes Toward Production, 1900–1932* (Ithaca: Cornell Studies in Industrial & Labor Relations, Vol. II, 1952).

was pitted against every other worker in a wild competitive scheme to make more and earn more. When Taylor would illustrate his philosophy with examples like those of the pig iron handlers who, in return for an increase in output on a hand paced operation of 400 per cent, were rewarded with a 40 per cent increase in wages, the fury of the trade unionists waxed hot. The issue was formally joined when the government introduced the Taylor Premium Pay Plan at the Watertown arsenal in Massachusetts in 1909. The unionized molders struck and were immediately supported by the American Federation of Labor. Gompers wrote vitriolicly in the *American Federationist:*

> Systematization in getting materials ready for the ultimate workman on the final job is not novel but scientifically building up the skilled mechanic himself . . . molding, hammering, filing and polishing him off in order to fit him for his theoretically best usefulness—that charms us unto the very soul.[7]

Taylor insisted that:

> An establishment running under the principles of scientific management will confer far greater blessings upon the working people than could be brought about by any form of collective bargaining.[8]

Taylor insisted that the distribution of the product among labor, the consumer, and the public could be done on an objective scientific basis. This confusion of scientific concepts with value judgment was behind much of the antagonism which Taylor generated among trade unionists. The promotion of scientific management systems to managers as substitutes for trade unions made this a matter of life and death with the unions.

In 1910 Louis D. Brandeis, later a Supreme Court Justice, introduced the testimony of one of the leaders of the scientific management movement, Harrington Emerson, into a case before the Interstate Commerce Commission. Mr. Brandeis was appearing on behalf of a group of shippers who were protesting

[7] *Ibid.*, p. 16.

[8] F. B. Copley, *Frederick W. Taylor* (New York: Harper & Bros., 1923), Part II, p. 416.

the application of the railroads for an increase in freight rates. Mr. Emerson testified that the railroads could save themselves $1,000,000 per day by the adoption of scientific management techniques. This made the scientific management movement-labor movement conflict front page news and led to a number of government investigations. The proceedings before these public bodies gave both sides an opportunity to spell out the issues and their respective positions.

The first of these bodies was an investigating committee of the House of Representatives. The purpose of the committee was to determine whether or not the techniques of scientific management should be permitted in government arsenals and navy yards.

The following exchange before the committee points up the conflict between the exponents of scientific management and the leaders of organized labor.

The Chairman: The other day Mr. Taylor, you made the statement that the mechanism of scientific management was a power for good and a power for bad.

Mr. Taylor: Yes, sir.

The Chairman: Now, if scientific management is a power for good and a power for bad and scientific management requires that there shall be only one directing head, with no interference with the law of that directing head, how is the workman going to protect himself against the power for bad that is in that system?

Mr. Taylor: Why, that is not scientific management, Mr. Chairman. I have tried to point out that the old fashioned dictator does not exist under scientific management. The man at the head of the business . . . is governed by rules and laws. . . .

The Chairman: If the enforcement of a law, however, is dependent upon the will of a man who has the power to violate it, there is not much likelihood of the law being enforced against him is there?[9]

Organized labor could not have expressed its view any more clearly than this government representative. Some four years later a rider was attached to a naval appropriations bill prohibiting the use of government funds for stop watch studies in government plants.

[9] F. W. Taylor, *Scientific Management* (New York: Harper & Bros., 1911), pp. 188–189.

In 1914 the United States Commission on Industrial Relations undertook still another investigation of scientific management as one phase of its total investigation of United States industrial relations. When Carl Barth, a leading associate of Taylor's, was called to the stand, the following exchange took place between him and the chairman.

Mr. Thompson: Then you have not taken into consideration the question of collective bargaining?

Mr. Barth: No; not very seriously, because I do not shake hands with the devil.[10]

Attitudes like these were hardly designed to endear the scientific management movement to the labor unions.

Professor Robert R. Hoxie of the University of Chicago was requested by the Commission on Industrial Relations to undertake an investigation of whether or not there were any possibilities of reconciling the scientific management movement with the organized labor unions. Professor Hoxie was joined by John P. Frey, editor of the A. F. of L. *Molders Journal,* and Robert G. Valentine, a management counselor.

The labor movement objections to scientific management that were examined by the investigators may be divided into two categories: conflicts over techniques involving pure difference in measurement, and conflicts implied in different interpretations of the workings of our economy. Among the measurement objections may be listed the following:

It [scientific management] looks upon the worker as a mere instrument of production. . . .

[It] stimulates and drives the workers up to the limit of nervous and physical exhaustion. . . .

[It] holds that if the task can be performed it is not too great. . . .

It tends to set the task on the basis of "stunt records of the strongest and swiftest workers without due allowance for the human element or unavoidable delays. . . .

It ordinarily allows the workman no voice in . . . the setting of the task . . . or the general conditions of employment. . . .[11]

Note that every one of these objections is a measurement objec-

[10] McKelvey, *op. cit.,* p. 19.

[11] Hoxie, *op. cit.,* pp. 169–171.

tion; that is, a rejection of the management method of measuring a task.

Now consider another set of objections based upon a conflict in ideas about the automatic distribution of the fruits of high productivity:

> It leads to overproduction and the increase of unemployment. It puts into the hands of employers at large an immense mass of information and methods which may be used unscrupulously to the detriment of the workers . . . and offers no guarantee against the abuse of its professed principles and practices. . . .
>
> It forces the workers to depend upon the employers' conception of fairness, and limits the democratic safeguards of the workers.

and then finally:

> It concerns itself almost wholly with the problem of production, disregarding, in general, the vital problem of distribution. . . . [It] violates and indefinitely postpones the application of the fundamental principle of justice to distribution.[12]

If the Hoxie Commission did not bring about a reconciliation between the scientific management movement and the trade unions, it did at least clearly spell out the issues. The Hoxie Report can be said to close the first period of unmitigated antagonism between organized labor and the scientific management movement.

A dissident group, however, within the scientific management movement, had begun to express itself somewhat earlier than the Hoxie Report.

The groundwork for a transition to a more viable relationship between these two currents of thought was being laid by men like Henry Gantt, Robert G. Valentine, and Louis D., later Justice, Brandeis. Brandeis had declared in his original brief, submitted in 1910 in the Eastern States Railroad Rate Hearing, that "Collective bargaining is alike an important function under scientific management and under the old system." The following exchange between Brandeis and Gantt took place at the hearing. Gantt was an expert witness.

[12] *Ibid.*, pp. 170–173.

Mr. Brandeis: But it is, as Commissioner Clark has pointed out, perfectly possible, and indeed very probable, that there would always be two sides to the bargain?

Mr. Gantt: Yes, sir.

Mr. Brandeis: Because you would have to fix the terms where the bonus would begin, and the amount of the bonus, and therefore collective bargaining in the sense of having a union to represent the working men, would . . . be just as necessary and just as beneficial in its operation as it is today.[13]

Henry Gantt had shown some suspicion that the mere institution of scientific management would not necessarily lead to unlimited production. As early as 1910 he observed that the administrators of some of these techniques seemed to be more interested in making prices than they were in making products.[14]

It was in 1915, however, that Robert G. Valentine published his classic paper, "The Progressive Relationship between Efficiency and Consent." He stated that "Under constitutional industrial relations, they [the unions] will contest the share in the management and the share of the product between themselves and the consumer."

Separating the problems of scientific management into two categories, he called the Taylor Society to provide a planning department to treat two classes of problems:

1. Those relating to the determination of the best way of performing an operation under a given set of conditions.
2. Those relating to the social, industrial and moral effects of putting into operation the organization or methods which scientific investigation has determined to be technically the best.[15]

Finally, he defined the relationship between efficiency and consent—the doctrine that the worker individually and in organized groups has a right to share in the determination of the conditions under which new technical methods and apparatus shall be put to use.

Shortly thereafter, Valentine and Brandeis received an oppor-

[13] L. D. Brandeis, *Scientific Management* (New York: *Engineering Magazine*, 1911), p. 55.

[14] H. Gantt, "Work, Wages and Profits," *Engineering Magazine*, 1911, p. 231.

[15] *Bulletin of the Taylor Society*, January 1916, p. 9.

tunity to apply their techniques experimentally amid a good deal of hope and optimism.

In January, 1916, a board of arbitration of which Louis D. Brandeis was a member handed down an award revising "A protocol of peace governing the relationship between the Dress and Waistmakers Association of New York City and the International Ladies' Garment Workers' Union." The award provided for a board of protocol standards charged, among other duties, with supervising the making and testing of piece rates and the assignment of standard times to different operations in the garment industry. Robert G. Valentine was chosen the first chairman of this board. The board was set up in March, 1916, but by September disagreements and rejection of its finding by the employers' association led to complete ineffectiveness. By 1917 we were in World War I, Valentine had died, and a new era in the importance of organized labor in national affairs had begun.[16]

World War I opened up new opportunities to leaders of the labor movement. President Wilson instructed the Secretary of Labor to set up a Federal board of arbitration. The Secretary of Labor set up a war labor conference board on which there were an equal number of representatives from the National Industrial Conference Board and the American Federation of Labor. The conference suggested the organization of a National War Labor Board. The new agency was governed by the following principles:

1. The right of workers to organize in trade unions and to bargain collectively through chosen representatives is recognized and affirmed.
2. Employers should not discharge workers for membership in trade unions nor for legitimate trade union activities.[17]

These measures lent labor a degree of security. Finally, when Samuel Gompers himself was chosen a member of the advisory commission of the Council of National Defense, it meant that

[16] L. Levine, *The Women's Garment Workers* (New York: Huebsch, Inc., 1924), pp. 306–309.
[17] McKelvey, *op. cit.*, p. 33.

the scope of his activities reached outside of the immediate employer-employee relationship. Other labor leaders were called upon to serve on various policy setting boards. In addition, a friendship developed between Morris L. Cooke and Samuel Gompers. Morris L. Cooke was one of Taylor's original associates and prominent in the affairs of the Taylor Society and the American Society of Mechanical Engineers. It was this social contact that led to the reconciliation between the leaders of the scientific management movement and the organized labor movement. The experience that organized labor had developed in its participation in joint labor management committees led the movement to look forward to a constructive relationship between itself and management following World War I. Most of management took its leadership from Elbert Gary of United States Steel at that time. Strangely enough, the group around the Taylor Society became a sort of left wing management group which continued to develop an experimental approach to the theory of organization and administration of industrial enterprises and labor participation in these functions.

In 1919, the year following the termination of the war, two developments took place. First, President Wilson called an industrial conference to enable labor and management to set out a policy that would promote production and preserve industrial peace. The second was that Samuel Gompers agreed to edit jointly with Morris L. Cooke and Fred Miller, President of the American Society of Mechanical Engineers, a set of papers expressing the points of view of the industrial scientists and the representatives of organized labor. This volume was published by the American Academy of Political and Social Science.

The contrast in the approach and treatment of the problem by the management participating in the President's Conference on Industrial Relations and the group editing the *Annals* is perhaps best understood with reference to the material published by each of the groups.

The President's Industrial Conference broke up when management insisted upon the following resolution:

Resolved: that without in any way limiting the rights of wage earners to refrain from joining any association or to deal directly with the employer as he chooses, the right of the wage earners . . . to bargain collectively with employers is recognized. The right of the employer to deal or not to deal with groups of men who are not his employees is recognized.[18]

By way of contrast, in the volume, *Labor Management and Production,* the engineers stressed the importance of giving labor a voice not only in collective bargaining but in industrial management itself.[19]

The overwhelming part of management took its cue from the Industrial Relations Conference statement and a great union busting campaign was undertaken which reduced the importance of the unions to a shadow of their World War I strength.

The publication of *Waste in Industry* in 1921 marked another milestone in bridging the gap between organized labor and the leadership of the scientific management movement. This volume was the outcome of a proposal made by Herbert Hoover to the Council of the Federated American Engineering Societies that a group of engineers conduct an organized survey of a number of chosen industries and make recommendations for the elimination of wasteful practices. The committee assayed the wastes in industry and made recommendations which called for the participation of labor in administrative decisions which would eliminate waste. In other words, instead of merely philosophizing about labor's participation in industry, it pioneered a blueprint for such participation.[20]

The leaders of organized labor protested in vain their newfound interest in production. As late as 1925 labor still proclaimed that:

There is still a more important service that the union can render, that of participating in finding better methods of production and greater production economies. A group of workers cannot enter into this type of co-

[18] *The New York Times,* October 17, 1919, p. 1.

[19] "Labor Management and Production," *Annals* of the American Academy of Political and Social Science, Vol. viii–ix, 1920.

[20] McKelvey, *op. cit.,* p. 69.

operation unless they know the results of their work will not be used to their disadvantage.

We recommend that the Federation keep in touch with such engineers and industrial experts as may be helpful in developing the information and procedure necessary to union management cooperation.[21]

Gompers was invited to make speeches to the American Society of Mechanical Engineers, and Green, his successor, to the Taylor Society. A number of interesting experiments emerged from this co-operation between management's left wing and the American labor movement, but by and large the twenties was an "ice age" for labor with an occasional warm island known as an experiment in union-management co-operation.

Under the influence of men like Morris L. Cooke, Otto Bayer, and Geoffrey Brown a number of joint experiments were taken up in the twenties. The Cleveland agreement between the International Ladies' Garment Workers' Union and the Cleveland Manufacturers Association, the Baltimore and Ohio experiment, and the Naumkeag experiment were the most heavily publicized of these ventures.

The agreement between the Cleveland Manufacturers Association and the International Ladies' Garment Workers' Union called for the setting of piece rates by time study techniques.

Under the Baltimore and Ohio experiment, committees of workers were set up independent of grievance committees to make operating suggestions in the maintenance shops of the railroads.

The agreement between the United Textile Workers Union of America and the Naumkeag textile mills in New England likewise called for the use of time study techniques to increase work loads.

The experiences which these unions accumulated with the technique were later to give rise to a positive development of a trade union philosophy of industrial engineering. The common

[21] 1925 Report of the Executive Council of the American Federation of Labor Convention, p. 32.

denominator shared by these experiments was that the unions frankly acknowledged an interest in increasing production, and administrative machinery was set up in each case to help the parties carry through their objectives.

All of the experiments had terminated by 1931 when the great depression made any concern with productivity by either management or labor appear completely obsolete. The course of the experiment during its successful phase left very little impression on American industry, though it did cause some commotion in the intellectual world. At the time of the demise of the experiments the fundamental industrial relations policies of large American mass production corporations remained anti-union, and it was they, rather than the intellectual, who dictated the business environment.

It is difficult to draw a fine line between the area where ordinary collective bargaining ceases and union-management co-operation begins. Collective bargaining itself starts with a pure conflict psychology and then in a developing relationship the conflict is attenuated and a new relationship develops out of the old. For example, John Mitchell, President of the United Mine Workers of America, in 1890 defined trade agreements as follows: "Joint agreements are in fact treaties of peace determining the conditions under which the industry will be carried on."[22] Note that the whole emphasis is on conflict, either active or suspended.

Ordinary collective bargaining begins to evolve into union-management co-operation, or an invasion of management prerogatives, depending upon your set of prejudices, when the union is ready to discuss subjects like production and sales outside the immediate employer-employee relationship. All through the twenties the American Federation of Labor emphasized its devotion to the concept of union-management co-operation. It frankly was using it in good faith as an organizing technique. The southern organizing drive of the twenties made its appeal to employers by offering the services of the Federation to re-

[22] J. Mitchell, *Organized Labor,* n.d.

duce wastes and increase work loads by setting up committees to give workers a democratic voice in decisions affecting them. President Green undertook a speaking campaign in the South in which he publicized the doctrine. He said:

> We offer to management and to the owners of industry a cooperative organization willing to give the best we have in training and service and skill in order that the industry may be made profitable.

Geoffrey Brown, an industrial engineer, served on the Federation's staff and held innumerable conferences with southern textile manufacturers. He wrote to one of them:

> I would like to emphasize to you again that the union-management co-operation plan that I am prepared to develop is constructive in character and is designed to place the working force four squarely behind production in such a way that a high hourly output per worker and a minimum unit labor cost will be maintained uniformly throughout the mills. The plan contemplates the organization of a joint cooperative committee to confer on such matters as . . . scientific extension of the number of looms or spindles . . . per operator . . . and any other matter in which the effectiveness of the worker influences the prosperity of the mill.[23]

The campaign won virtually no buyers, and the Federation, along with the rest of the labor movement, turned toward more classical organization techniques when the passage of the National Industrial Recovery Act in 1934 made the organization of workers once more feasible. These experiences with union-management co-operation influenced very deeply the attitudes of the growing trade unions towards productivity as a concept and industrial engineering techniques as tools of collective bargaining.

It is already obvious from what has been said that the concept of union-management co-operation did not carry enough appeal to sell unionism to employers except in some very exceptional cases. Both the Naumkeag experiment and the Cleveland Garment Workers' experiment broke down because the depression made it impossible for workers threatened with unemployment to see any particular advantage in participating in high pro-

[23] McKelvey, *op. cit.*, pp. 105–106.

ductivity schemes. The railroad experiments continued on a much reduced plane and with a declining enthusiasm for the same reasons.

The new unions which were growing rapidly and the old unions which were increasing their membership at a very rapid rate were formulating, by their activities, their own philosophy of production. The writer attempted to outline this philosophy at a public forum. Although it may not have ever received formal expression, it nevertheless portrays the thinking of the trade unionist on the subject of productivity.

The question of productivity is posed more as a challenge than as a search for dispassionate information. The challenge generally proceeds on the implicit assumption that, by and large, management is for high productivity, and that, by and large, labor is for restricted productivity. The role of a few progressive unions is acknowledged. This is supposed to point up the contrast with what is considered the more common restrictionist behavior.

The truth is that both management and labor view this question of high productivity from different angles. Management is interested in the intensive view of high productivity. Labor, on the other hand, is interested in the extensive view of high productivity.

When management says they want high production, they mean that they want the highest production that they can possibly get during those hours when they find it profitable to permit the workers to operate the machines of which they are the custodians. Should they find it unprofitable to operate these machines, then they would be considered very unwise managers indeed if they went on turning out merchandise classed in accounting language as idle inventory. In fact the efficiency of the manager will be judged in terms of whether or not he is wise enough to shut down his factory in time to avoid accumulating an idle inventory. This behavior is considered sound business practice and is socially acceptable. Actually, it is a wanton waste of our irreplaceable man-power resources.

Now take a working man. He's been conditioned by his historical fear of unemployment. He finds through the factory grapevine that, at most, six weeks of work is left in the factory. His problem is: what do you do at the end of six weeks? Well, it would be a lot easier if you didn't have to face the problem for seven weeks. And so he will resort to devices to make six weeks' work last seven weeks. Immediately aimed at his head is the accusing finger labeling him "featherbedder" and "victim of the make-work fallacy." He's interested in high production, but he calls it full employment.

This is what I mean by the contrast between the worker's interest in

the extensive view of high productivity and management's interest in the intensive view of high productivity.

Obviously both management and labor are reacting normally to the logic of their experience. Hortatory harangues about the necessity for high production in the abstract just bore everybody.

The union institution must resolve two contradictory forces which the members press upon the leadership. Your membership wants at the same time a defensive safeguard against technological innovation and simultaneously some of the gains that come from that same innovation. Obviously, in an expanding economy the union can emphasize the gains available. It is not afraid of unemployment.[24]

This last paragraph, of course, is the key to the union's behavior in a local situation. Joe Scanlon and Clinton Golden, formerly Research Director and Assistant to the President of the Steel Workers' Union and more recently members of the faculties of the Massachusetts Institute of Technology and Harvard University, respectively, have had dramatic success with some union-management co-operation plans for which both of them have been responsible.

A careful analysis of the application of the Scanlon Plan in the La Pointe factories and the Stromberg-Carlson factories discloses nothing very unique except the development of a new human relationship between the management and the workers under the leadership of a very forceful personality like Joe Scanlon. What made it possible, however, for Mr. Scanlon to exert this influence on the principals was the expanding economy of the country in 1950. The workers' fear of unemployment is at a minimum and Mr. Scanlon's persuasiveness has led the management to permit the union to participate in a whole host of activities which would be listed elsewhere as management prerogatives. The particular group incentive payment plan in which workers augment their wages by attempting to raise productivity by reducing actual costs below a standard cost has been used elsewhere with mixed results. It would therefore be a mistake to confuse the mechanics of the Scanlon

[24] W. Gomberg, "Release the Brakes on Output," New York *Herald Tribune Forum,* October, 1950.

Plan of union-management co-operation with the effective ingredient which is Mr. Scanlon's personality.

Most of the so-called War Production Committees or Nelson Committees as they were called, named after Donald R. Nelson, Chairman of the War Production Board, never really functioned despite the plethora of publicity with which they were surrounded. This was due to the fact that management looked upon these committees with suspicion for two reasons.

1. They would increase the power of the unions.
2. They permitted the unions to invade areas of management which were still the subjects of controversy. Management maintained, for example, that the application of many industrial engineering techniques remained a unilateral function of management.

It is thus interesting for the trade unionist to watch the ambivalence of management behavior. First management deplores the union's lack of interest in subjects like high productivity and its attendant techniques like accounting, time study, rate setting, and so forth. When the union is ready to discuss these subjects in collective bargaining, it is likely to be accused of invading management prerogatives.

The principal problem faced by trade unionists in coping with management techniques was first to have the fact recognized that the techniques of the industrial engineer belonged in the area of collective bargaining. In the course of developing this case an entire philosophy of trade union industrial engineering was developed. It does much to explain current labor movement attitudes toward time study specifically.

CHAPTER 2

The Time Study Problem Today

The close interrelationship between the fields of industrial engineering and collective bargaining requires a re-examination of some of the techniques of the profession which have hitherto gone effectively unchallenged.

Although academic discussions may have taken place in the past about the relative accuracy of these techniques, it has been only in the last decade that the growth of the labor movement has made an investigation of these problems imperative. One of the most important techniques of industrial engineering affecting the collective bargaining process is the time study procedure for rate-setting purposes.

The next few pages recount the development of this problem in the labor union with which I am associated. In order to place the problem in its true setting and to permit an organized outline of its development, a short autobiographical sketch is included, briefly outlining my experiences in time study.

It was in 1933 that I was graduated from the College of the City of New York with a B.S. degree in Chemical Engineering. The bank holiday was around the corner and I was more than happy to get a fifteen-dollar-per-week job as a shipping clerk in the ladies' dress industry. It was not long before the National Industrial Recovery Act stimulated the first union organizing wave that swept the garment workers into the Inter-

national Ladies' Garment Workers' Union, me along with the others.

I was soon pressed into union service, and I have remained there ever since. I first served as secretary of the Eastern District Department of the union. The department was charged with the negotiation and administration of union agreements in factories located in the New York City suburban area. Later I was assigned the task of administering part of the collective agreement governing the relationship between the employers and employees of a number of children's dress factories located in New York City and attached to the jurisdiction of Local 91 of the International Ladies' Garment Workers' Union. This work was under the supervision of the vice-president in charge of the local.

The administrative problems in both of these situations were identical. Both contracts provided for a piece-rate method of wage payment to the members of the most numerous crafts employed in the factories, the pressers and sewing machine operators. The members of these crafts made up the overwhelming proportion of workers employed in the factories. The problem of setting mutually satisfactory piece rates occupied the major part of the administrative officers' working time.

Agreements with new firms generally specified the acceptance of existing piece rates plus a percentage increment on present styles in operation. New styles making up the bulk of future work were left to joint determination by rate committees of workers and representatives of the management.

In the event of a disagreement the business agent, that is, the local administrative officer, would be called upon to assist the members of the union committee in settling the dispute. The business agents for the most part had been either pressers or sewing machine operators. They had a wealth of empiric knowledge that enabled them, after a good deal of haggling and bargaining, to reach a mutually satisfactory rate. After one of these sessions all parties would be completely exhausted and, more often than not, suffering from nervous indigestion. Experiences

like these led to jokes about stomach ulcers being the occupational disease of union officers.

This pragmatic knowledge of analysis by both management and labor was not as unsound as at first glance seems to be the case. Most of the business agents remembered vague bits of operations upon which they had worked and approximately how much time the operation should take. Others had picked up some knowledge from their constant contacts in other factories and negotiating sessions. Needless to say, this old approach resulted in earnings that varied widely for the same people from week to week. However, at no time was any systematic attempt made to collect the requisite knowledge of production standards in any organized form or, for that matter, to reduce it to writing.

This in itself, aside from the exasperations of the old approach, was a challenge to any trained newcomer among the business agents to seek a more rational way of settling rates. A general prejudice against the choosing of non-craftsmen as business agents sprang from a conviction that only an operator understood how long it should take to sew a garment and a presser how long to press a garment. Officers who were formerly pressers or operators would attempt to overcome their restricted information by exchanging notes on an informal basis so that they could service any craft. The limitations of such a procedure are at once apparent. They provide the atmosphere within which any new-fangled reforms would have to justify themselves.

While seeking some techniques as a substitute for this nerve-wracking method of negotiating by contrary pressured guessing, another division of the same international union, the Dress Joint Board, charged with the administration of collective agreements in the New York City and suburban ladies' dress industry, announced the adoption of the "unit system."

The unit system defined a unit as one-tenth of a minute. It then provided for the breakdown of all styles into standard parts of all dresses and operations common to the manufacture of each. This was followed by the systematic codification of these operations into a reference file accompanied by the "scien-

tifically" determined unit-times that a normal operator should take to complete the operation. Somehow the use of the standards of the unit system to build up synthetic rates did not lead to the expected results. Wide disparities in earning opportunities on different styles persisted. It was not long before the form of the unit system was maintained but its essential substance abandoned.

Old-line business agents derived a certain sense of relief and impish satisfaction from the failure of the plan to come up to expectations. Their openly-displayed contempt expressed itself in such remarks as, "So you expect a lot of youngsters with slide rules to set rates, huh? Did they ever sew a seam? It's all nonsense."

Many officers and engineers, however, concluded that, although the standards underlying some of the operations might require revision, the basic procedure, the use of time study, was sound.

Nobody at the time attempted to question the *validity of the basic time study techniques* underlying the standards. The scientific standing of the technique itself was not questioned. Unfortunately the pressure of immediate negotiations precluded the opportunity to test the accuracy of any of these suppositions. The unit system became a loose guide that set up more rational limits within which haggling continued to take place. It merely relieved the intensity of the emotionalism of the procedure.

At about the same time the New York dress unit system was being installed, industrial engineers appeared in the needle industries, particularly in places outside the older metropolitan manufacturing areas. They announced that with the assistance of scientific time study they could set production rates, challengeable by neither management nor labor. They wanted management and labor merely to fix an expected hourly wage. The engineers would key this wage to their scientifically set rates.

Strangely enough, in an effort to bolster arguments for this procedure, some of the engineers hailed in glowing terms the use of the unit system by the Dress Joint Board of the Interna-

tional Ladies' Garment Workers' Union. The description of the rigidity of the technique as they thought it was practiced was seldom recognizable to anybody participating in the process.

Carroll, maintaining that setting production times was a matter that should be left solely to the qualified engineer, bade both management and labor to keep hands off.[1] With so much at stake, neither party had heeded his well-intentioned advice and perhaps advisedly so, as will be indicated later.

The entry of industrial engineers into the rate-setting functions of the industry led the International Ladies' Garment Workers' Union to create a Management Engineering Department of which I am director. The department was charged, among other functions, with responsibility for the development and formulation of the union's rate-setting policy, subject to the local union officer's approval.[2] Quite naturally it was not long before the work of the department led to an examination of the very foundations of time study.

The unique place occupied by the Management Engineering Department in a trade-union administrative structure led to inquiries from other labor unions such as the United Automobile Workers of the Congress of Industrial Organizations and the Commercial Telegraphers Union of the American Federation of Labor. These inquiries in time led to requests for assistance in similar problems from others, so that rate setting came to be viewed by department staff members from the angle of many industries.

[1] Phil Carroll, Jr., "One View of Labor's Participation in Motion and Time Study," *Advanced Management*, Vol. VI, No. 2, April 1941, pp. 75–80.

[2] The objectives of the Management Engineering Department were listed as follows:

1. To assist in improving the manufacturing techniques and operating methods of all branches of the ladies' garment industry with which the earnings of our workers are intimately bound up. This will be done through plant inspections by department representatives, followed by specific recommendation.

2. To serve as a central information agency: (a) for the determination of the level of "fair piece rates"; (b) to record the production system and manufacturing techniques under which these rates are paid; (c) to assist in training shop members and committees in distinguishing bad time study practice from good time study practice, in the determination of rates. (Order of the General Executive Board of the International Ladies' Garment Workers' Union, July 22, 1941.)

Since 1946, the treatment of time study problems by trade unions has become much more formalized. The United Automobile Workers Union of the C. I. O. has organized its own engineering division of the research department under Nat Weinberg to deal with production standards disputes. The department has published booklets that instruct its membership and officers how to cope with the technique.

Sol Barkin of the Textile Workers Union of the C. I. O. heads an engineering division of the Textile Workers Research Department. The department has just issued an elaborate *Textile Workers' Job Primer* devoted to instructing its membership and officer corps in the intricacies of setting up equitable work loads.

The United Rubber Workers of the C. I. O. has a special corps of time study officers in the field who are trained to use time study as a collective bargaining tool.

The International Association of Machinists, A. F. of L., who historically have denounced any tolerance of time study, now have specialists on their staff whose function is to represent the workers in disputes over the application of time study techniques to the negotiating and collective bargaining process.

The International Confederation of Free Trade Unions, which includes in its membership representatives of all of the trade unions of the free world, expressed its attitude toward the use of time study for the setting of production standards at its Berlin Conference in July, 1952.

> The trade unions' attitude towards time and motion studies . . . will be determined according to the merits of each case. It will largely depend on the safeguards which the unions will be able to secure. In general, trade unions will make the following demands:
>
> (a) Trade unions must take part in the determination of what methods should be used, on what jobs and under what conditions.
>
> (b) Trade unions must participate in the control over the studies which are made and over the decisions as to the use of their results.
>
> (c) The results of the studies must in all events be only a starting point for negotiations between the employer and the union on such matters as wages and introduction of changes in the organization of work.
>
> (d) There must be proper safeguards against over-exertion. . . .
>
> (e) Workers must share, through better wage standards and improved

working conditions, in any increases in productivity brought about by "scientific management."[3]

America's entry into World War II, accompanied by the necessity for heavily increased production at the time of a severely constricted industrial manpower supply, aggravated the whole issue of what was a fair day's work and focused attention on the methods by which scientific time study arrived at a correct estimate of the work load.

An examination of the files of the National War Labor Board of World War II will disclose case after case in which disputes over work loads had resulted in work stoppages.[4] Constant references to time study intersperse all of these disputes, but in no case is there an attack upon, or even an examination of, the time study technique as such.

Things reached such a pass that Edwin A. Lahey, writing in the *Pittsburgh Press* about the acute Detroit labor situation immediately before its explosion into the General Motors strike of 1945–1946, expressed the belief that behind most of the unrest on the local level were disputes over the measurement of a fair day's work.[5]

Reviewing present-day labor attitudes toward the whole question, we find a distinct departure from labor's attitude in 1915 when Hoxie wrote his *Scientific Management and Labor*.[6] At that time labor rejected time study as a nostrum, a fleeting fancy, a patent medicine that would disappear.

By the end of World War II many labor organizations had accepted time study in principle. Arguments continued, however,

[3] "Statement on Prices, Wages and Productivity," I.C.F.T.U. First General Council Meeting, Berlin, July 1–5, 1952, p. 3.

[4] For example: Case No. 111-16460-D, *Lockheed Corp.* vs. *International Association of Machinists;* Case No. 111-3692-D, *G. F. Richter Manufacturing Co.* vs. *United Electrical Radio and Machine Workers Union;* Case No. 111-8458-D, *Allis-Chalmers Manufacturing Co.* vs. *United Automobile Workers Union;* Case No. 83068-D, *Curtis Wright Corp.* vs. *United Automobile Workers Union;* and Case No. 111-1073-D, *Timken-Detroit Axle Co.* vs. *United Automobile Workers Union. War Labor Reports* (Washington, D.C.: Bureau of National Affairs).

[5] Edwin A. Lahey, *Pittsburgh Press,* September 25, 1945.

[6] Robert Franklin Hoxie, *Scientific Management and Labor* (New York: D. Appleton & Co., 1915), pp. 169–177.

over participation in the technique and the validity of conclusions that could be drawn from specific studies. One of the arguments was over the question: Is time study a subject for collective bargaining? The whole problem of the validity of time study techniques had been pointed up by a postwar counteroffensive of management against the unions. This was presumably an effort by management to recapture its prerogatives or essential function. This offensive showed itself most graphically at President Truman's labor-management conference in 1946. The president had called into conference the national leaderships of the National Association of Manufacturers, the United States Chamber of Commerce, the American Federation of Labor, the Congress of Industrial Organizations, and the Railway Brotherhoods. Among the subcommittees appointed by the conference was the subcommittee on "management's right to manage." This subcommittee was made up of an equal number of representatives of management and labor. The subcommittee was unable to come out with a joint report and two separate reports were issued: one by management and the other by labor.

The management members addressed a letter to the conference, extracts from which follow:

> Labor members of the committee on management's right to manage have been unwilling to agree on any listing of specific management functions . . . if labor disputes are to be minimized by the genuine acceptance by organized labor of the functions and responsibilities of management to direct the operation of an enterprise, labor must agree that certain specific functions and responsibilities of management are not subject to collective bargaining . . . the determination of job content; the determination of the size of the work force; the allocation and assignment of work to workers.[7]

Organized labor, on the other hand, argued that it would be extremely unwise to build a fence around the rights and responsibilities of management on the one hand and unions on the other. The experience of many years shows that with the growth of mutual understanding the responsibilities of one of the parties

[7] *President's National Labor-Management Conference, November 5–30, 1945,* United States Department of Labor, 1946, pp. 56–61.

today may well become the joint responsibility of both parties tomorrow.

The industrial engineering profession reflected and rationalized the more conservative management philosophy. For example, J. K. Louden warned that ". . . a standard must be based only upon facts and changed only by facts";[8] therefore, standards must not be subjected to negotiation or arbitration either in their establishment or in their change. Presumably the facts clothed in such austere dignity are the facts disclosed in the time study. Although Louden recognized the equity of the workers in the results of the study and recommended their participation, he avoided any specific recommendations or procedures.

Hill and Hook, who took a prominent part in the President's Conference, specifically excluded labor from participation in the time study process.[9] This, they claimed, was solely a management function.

This sort of thinking is somewhat at variance with the scientific objectivity to which the advocates of the time study technique aspired. Labor spokesmen stated that to argue over who shall participate in the making of an objective measurement of a task makes as much sense in industrial relations as a demand by a public utility that its customers not be permitted to read the gas or electric meter—such secrecy would breed suspicion in either case.

Yet many of the leading corporations, taking their cue from Hill and Hook, or perhaps merely carrying out an implicit policy of long standing now stated explicitly by Hill and Hook, maintained that setting the production standards was solely a management function. Although labor's right to question the standard might be entertained as a grievance, the grievance terminated with the management. It was nonarbitrable.

The position taken by management was that it would negotiate base rates with the union. If the system of wage payment

[8] J. K. Louden, *Wage Incentives* (New York: John Wiley & Sons, Inc., 1944), pp. 161–162.

[9] Leland H. Hill and Charles R. Hook, Jr., *Management at the Bargaining Table* (New York: McGraw-Hill Book Company, Inc., 1945), p. 123.

was measured by day work, management reserved the right to fix the work load expected. If the system of wage payment was some incentive payment plan, management reserved to itself the right to fix the production standard underlying the incentive earning opportunity.

Organized labor, in turn, took the position that it was concerned with the amount of work expected from its membership under measured day work and with the earning opportunities afforded its followers under a wage incentive payment plan.

The question of labor's participation in the setting of production standards is now pretty well settled. The most important case in establishing this right was the arbitration award settling the strike against an alleged speed-up of the Ford production workers in the River Rogue plant against the Ford Motor Company. The contract between the Ford Motor Company and the U. A. W.-C. I. O. at that time specifically gave management the unilateral right to determine production standards. The arbitration award handed down in the case, signed by the chairman of the arbitration board and the labor member of the board recognized "the right of the company to establish and determine and to maintain and enforce standards of production." (Sect. 4, Art. IV.)

The arbitration award stated that the "right of the company," which is "fully recognized" in Section 4, is not . . . like other "rights" . . . , as for example, the right to "decide the number and location of plants," or the "products to be manufactured," or the "schedules of production." . . . (Page 11, arbitrator's decision.) With regard to these matters, the company may make final determinations which the union must accept for the term of the contract. Such is not the case with respect to production standards. There the right "to establish and determine and to maintain and enforce" is more in the nature of a right to *initiate*.

The importance of this spelling out of management's right to determine production standards is not meant to imply that the Ford Motor Company practice governs the field of collective bargaining; however, the definition of the nature of this right in a basic industry of the American economy has influenced

thinking in all fields of collective bargaining. Before this decision established the nature of labor's interest in the setting of production standards, many progressive managements, although they did not go as far as the clothing industry, did attempt to resolve this problem by inviting labor's review of the time studies underlying the incentive rates. The most notable experiment of this type was the agreement between Local 2 of the United Automobile Workers, C. I. O., and the Murray Body Corporation in Detroit at the time that L. Clayton Hill was general manager.[10]

The union stewards were trained by the engineering consulting firm, Stevenson, Jordan, and Harrison, to take time studies in accordance with its basic time study technique. Conversations with administrative officers of Local 2 disclosed that this arrangement did not bring to an end rate disputes between the corporation management and the workers. No time study technique can end all disputes between management and labor. All that it can do is provide a frame of reference within which the dispute can be examined. All the claims by engineers for their pet procedures rest upon an assumed validity of existing time study practice well within the percentage increment or decrement to the wage scale involved in collective bargaining negotiations. Obviously, if, after months of negotiations and possibly strikes at great financial sacrifice to both sides, a settlement has been reached involving a ten per cent change in the basic rates, neither management nor labor is prepared to sacrifice its respective rights to the blind operations of a technique of questionable accuracy. The use of a time study technique to set production standards whose demonstrated inaccuracy may exceed this percentage can become the source of much controversy. Naturally the demand by either side to monopolize the function arouses the suspicion of the other. Thus the solution to the basic problem of the vailidity of existing time study practice lies at the very heart of satisfactory industrial relations. But the importance of time study transcends even this critically important sphere

[10] *Production Standards from Time Study Analysis,* Murray Corporation of America and the U. A. W., Detroit, 1942.

of activity. The imperfections of measurements lead to other disagreements.

The controversy over the percentage increase in the productivity of the country highlights the problem. Document No. 27 of the War Production Board set the increase in productivity of the country from 1941 to 1944 at 20 per cent.[11] This figure was severely questioned by managements interested in resisting wage increases.

The Bureau of Labor Statistics maintained that there had been no increase in the productivity of civilian industries during approximately the same period.[12] This time it was the representatives of organized labor who became enraged. They were interested in using the War Production Board's figures to justify wage increases. Here again, the dispute is traceable to the imperfect methods of measuring the relative productivity of the country in terms of highly variable dollars subject to inexact corrections. There is no doubt that the sound solution of the fundamental time study problem might lead to the development of standard labor units. The relative importance of these time units could be keyed to a job grading structure to reflect the relative skills involved. The assignment of definite numbers of labor units to manufactured products, once universally accepted, could lead to much more accurate indices of relative productivity over periods of time—if the time study technique can stand the test of scientific validity. The need for an improved technique of measuring productivity was pointed up during the operation of the Marshall Plan. The United States made a tremendous investment in restoring the Old World's productive capacity. When the United States undertook responsibility for restoring the factories of Europe, many government officers were aware that the United States would be held responsible for the social pattern of distribution among producer-con-

[11] *American Industry in War and Transition, 1940–1950,* United States War Production Board, Document No. 27, Part II (Washington, D.C.: United States Government Printing Office, July 20, 1945), p. 9.

[12] *Productivity Trends in American Industries,* Chart 1, United States Department of Labor, Bureau of Labor Statistics (Washington, D.C.: United States Government Printing Office, January, 1946).

sumer-workers that resulted from the new economy. One of the stumbling blocks to evolving a sound plan was the very limited nature of existing productivity measurements.

Murray and Cooke have questioned the time study as the basic method of compensating labor, pointing out that production standards based upon time study imply reward for a repetitive pattern of uniform physical and mental exertion.[13] They go on to point out that the whole modern tendency is toward automatic machine production with an increasing number of workers becoming mere machine tenders with little control over the pace of production.

The importance of this "automation" movement has been pointed up by new developments since Murray and Cooke completed their book[14] in 1940. Norbert Weiner's *Cybernetics*, published in 1948, gave much of the theoretical foundation for the new manless factory. Application of these principles by the Ford Motor Company in their new motor plants in Cleveland led to fantastic reductions in the number of workers required to turn out motors. Those who were left were mainly gadget repairmen and machine controllers.

Although this development is of importance in pointing up new economic requirements in the changing nature of industrial skills and in its resulting impact upon our industrial pattern, it still leaves the work requirements of the machine tender the new basic measure of the productivity of labor. The validity of the time study technique underlying this work measurement will determine the accuracy of the new index of labor productivity even though it may lose some of its importance as a method of compensating labor.

Performing so basic a function, the accuracy of the technique affects the stability of the very core of our industrial system. Confidence or lack of confidence in the technique will stop or start crippling strikes. A solution to the basic problem can be a major contribution to industrial peace.

[13] Morris L. Cooke and Philip Murray, *Organized Labor and Production* (New York: Harper & Brothers, 1940).
[14] *Ibid.*

CHAPTER 3

Time Study in the Field of Scientific Systems

Is time study a science?

The claims of rate setting to be scientific procedure can best be evaluated by an examination of the nature of the scientific method, its interpretation of the universe, and the changes which both have undergone throughout the years. Most hard-headed scientists, more particularly hardheaded engineers, may express some impatience with what they feel is a philosophical, unsubstantial approach. Large groups of technical specialists who have borrowed their methods from their teachers as a matter of rote point to the purely pragmatic results of their work and question all attempts to delve very deeply into the obvious.

This attitude, although it may lead to widespread acceptance of a specific technique, seldom adds anything of real permanent value to new thinking in the field which can lead to the eventual development of far sharper tools than the profession possesses at the present time. It is strange that in an age as scientific as ours there are so few scientists. True, we have a great many scientific workers who have memorized a specialized procedure within their own narrow specialized field; but there are few scientists who really appreciate the deep implications of the method which they are using and the obligations the method im-

poses upon its practitioners. Professor Cohen admirably sums up the present-day status of most scientific pretense when he writes:

> For that matter, most of popular science is just a new form of superstition. What evidence is there that, because a man has read something about the romance of the atom, he really understands the world better; that he has attained a more scientific turn of mind? What evidence is there that because a man talks freely about psychology, or psychoanalysis and complexes and libidos and things of that sort, he really has scientific detachment and a sense of scientific evidence and scientific method? I should say that changes of lingo and various exercises of technical vocabulary do not indicate any growth of science—though the body of knowledge available today is larger than it was. People who want to use the material of science certainly have a better chance. But that does not mean that the great body of people today are more scientific than they were before.[1]

Professor Cohen might even have added the further stricture that people who are unaware of the full implications of the scientific method may often do violence to the material of science while believing they are being scientific. The importance of this misunderstanding of the scientific method in time study practice will become apparent as our study continues.

One of the ways in which this situation can be remedied is by a re-examination of the nature of the scientific method itself, its course of development, and the present criteria that have been developed for testing the conformity of a method of investigation to scientific requirements.

The practical scoffers will again express impatience in the name of common sense. George de Santillana answered them brilliantly when he wrote:

> Many of the workers of science never care to consider the forms of thought and the consequences which the greater minds among them envisage. But when the wind sweeps over the wheat, the ears bend to the gusts without knowing the rhythm of the wind.[2]

[1] Morris R. Cohen, *The Faith of a Liberal* (New York: Henry Holt & Co., 1946), p. 451.

[2] George de Santillana and Edgar Zilsel, "The Development of Rationalism and Empiricism," *International Encyclopedia of Unified Science* (Chicago: University of Chicago Press, 1941), Vol. II, No. 8, p. 4.

The "practical scientists" seek to justify their revolt against rationalism by treating the experimentalism of Roger Bacon and the development of the practice into a standard philosophy by Francis Bacon a century later as a completely new phenomenon. Reasoning or rationalism becomes dangerous intellectualizing only when claims that violate evidence of observation are made for the method. As Cohen observed:

> Wisdom does not come to those who gape at nature with an empty head. Fruitful observation depends not as Bacon thought upon the absence of bias or anticipatory ideas, but rather on a logical multiplication of them so that having many possibilities in mind we are better prepared to direct our attention to what others have never thought of as within the field of possibility.[3]

Quite obviously nobody sits around observing facts without any pattern. The collection of facts seldom begins before a hypothesis is developed. It is this hypothesis that indicates the facts for which we will search, the observations which we will make, and the experiments that we will design.

Bacon, in seeking to overthrow the nonrational authoritarianism of the scholastics, virtually attempted to destroy rationalism as such. Hence, the current suspicion of theorizing in many hardheaded scientific circles. Attempts have been made to justify this revolt against theorizing by representing science as empirical rather than rational. Somehow it is forgotten that without reasoning, that is drawing rigorous logical inferences, empiricism becomes meaningless and science disappears.[4] Bacon confused the scientific method as a means of gathering facts with a method for testing facts. As Randall and Buchler have made plain: "What distinguishes the method of science is the manner in which its theories become transformed into knowledge."[5]

The answer to the superficial cry of "begin with the facts"

[3] Morris R. Cohen, *Reason and Nature* (New York: Harcourt, Brace & Co., 1931), p. 17.

[4] *Ibid.*, p. 5.

[5] John Herman Randall and Justus Buchler, *Philosophy* (New York: Barnes & Noble, 1942), p. 59.

by the advocates of the scientific management movement has been given by Professor Cohen:

> It is easy for those who have not reflected on actual scientific procedure to say: Begin with the facts. But an even more fundamental difficulty faces us. What *are* the facts? To determine them is the very object of the scientist's investigations, and if that were but the beginning or first stage of science, the other stages might be dispensed with. To determine the facts scientifically, however, is a long and baffling enterprise, not only because the facts are so often inaccessible, but because what we ordinarily take for fact is so often full of illusion.[6]

> The positivists who boast that they are concerned with only what is, like the hard-hearted statesmen or businessmen who say that they deal only with hard actualities, are deluding themselves with fantastical dogmas, hiding the crudity of their ideals with the pretense that they have none.[7]

It is these illusions made up of artificial transplantations from the concepts of the other sciences that have bedeviled time study techniques.

The manner in which this transplantation has taken place is at once apparent from an examination of the problem presented by rate setting. Taylor's directions remain primarily unchanged today. The work of the Gilbreths and their successors has essentially been a refinement of these original directions. Let us examine these directions for undemonstrated assumptions.

Taylor states that time study consists of two broad divisions, first, analytical work, and second, constructive work. The analytical work of time study is as follows:

> (a) Divide the work of a man performing any job into simple elementary movements.
> (b) Pick out all useless movements and discard them.
> (c) Study, one after another, just how each of several skilled workmen makes each elementary movement, and with the aid of a stop watch select the quickest and best method of making each elementary movement known in the trade.
> (d) Describe, record and index each elementary movement, with its proper time, so that it can be quickly found.

[6] Cohen, *Reason and Nature*, pp. 77–78.
[7] Morris R. Cohen, *Preface to Logic* (New York: Henry Holt & Co., 1944), p. 17.

(e) Study and record the percentage which must be added to the actual working time of a good workman to cover unavoidable delays, interruptions, and minor accidents, etc.

(f) Study and record the percentage which must be added to cover the newness of a good workman to a job, the first few times that he does it. (This percentage is quite large on jobs made up of a large number of different elements composing a long sequence infrequently repeated. This factor grows smaller, however, as the work consists of a smaller number of different elements in a sequence that is more frequently repeated.)

(g) Study and record the percentage of time that must be allowed for rest, and the intervals at which the rest must be taken, in order to offset physical fatigue.

(h) Add together into various groups such combinations of elementary movements as are frequently used in the same sequence in the trade, and record and index these groups so that they can be readily found.

(i) From these several records, it is comparatively easy to select the proper series of motions which should be used by a workman in making any particular article, and by summing up the times of these movements, and adding proper percentage allowances, to find the proper time for doing almost any class of work.

(j) The analysis of a piece of work into its elements almost always reveals the fact that many of the conditions surrounding and accompanying the work are defective; for instance, that improper tools are used, that the machines used in connection with it need perfecting, that the sanitary conditions are bad, etc. And knowledge so obtained leads frequently to constructive work of a high order, to the standardization of tools and conditions, to the invention of superior methods and machines.[8]

A glance over the ten steps reveals the subtle acceptance of a purely mechanical view of nature. Here the substance, the mechanical concepts with which science had been made to work in the field of nineteenth century physics, was artificially transplanted into the field of time study techniques. The fact that the only claim that could be made for these assumptions was that they were subject to tests of validity by the scientific method was confused with the superficial resemblance between the directions to divide a man's work into simple elementary motions and the appearance of the atomic and molecular theory of physical science. Taylor's paragraph *i*, that the best

[8] "The Present State of the Art of Scientific Management," discussion by F. W. Taylor on the Majority Report of the Sub-committee on Administration of the A.S.M.E., Vol. XXXIV, 1912, pp. 1131–1230, quoted in Richard B. Leng, "Observational Error and Economy in Time Study" (Unpublished M.A. thesis, Department of Administrative Engineering, New York University, 1941), pp. 3–4.

elements be combined into a synthetic pattern arithmetically, completely overlooks the possibility that these atomic movements might combine—as hydrogen and oxygen do—to form a completely new compound—water—bearing little or no resemblance to the original constituents from which it was formed, rather than make up an additive set.

While Taylor, as late as 1912, was transplanting into time study the mechanical dogmas that had influenced nineteenth century physics, the whole structure of physical theory was changing and with it our whole concept of nature.

The Encyclopedists of the Age of Reason, in their revolt against dogmatic authority, had exchanged one set of dogmas for another.[9] This theory that the whole universe is a mechanical apparatus which acts strictly in accordance with a foreordained set of mechanical laws was a useful assumption in physics until the discovery of X rays by Roentgen in 1895. A series of discoveries—the disclosure of the properties of radium by the Curies, the development of relativity by Einstein, the unfolding of quantum mechanics by Planck, and the revelation of the indeterminacy principle by Heisenberg—exploded the mechanical view of the universe. Along with it should have gone many of the artificial extensions of the mechanical view of nature in the social sciences, but the same confusion of the scientific method with a higher dogmatism that was responsible for their first unjustified extension has been responsible for resistance to their elimination in many quarters.

A good example of this resistance is the talk that is still heard in many economic circles of both the left and the right about the immutable natural laws of economics or the laws of management. (See, for example, "The Unwritten Laws of Engineering" that was awarded a special prize in 1946.[10])

The mechanical deterministic view of the universe was now replaced by a statistical-probability picture. Hitherto, the applications of statistical methods to physical phenomena, since

[9] Cohen, *The Faith of a Liberal*, p. 430.

[10] W. J. King, "The Unwritten Laws of Engineering," *Mechanical Engineering*, Vol. LXVI, 1944, pp. 323–26, 398–402, 459–62.

the time of Laplace, had developed under the title of The Theory of Errors. The implicit assumption was that we were concerned solely with measuring the deviations of our imperfect measuring instruments. The mechanical view of the universe permitted no variation in the foreordained deterministic fixed values assigned any phenomenon.

The continual refinements of measurement ultimately led to the conclusion that the microscopic physical world was statistical in nature and that macroscopic phenomena merely represented behavior of a very high order of probability but not certainty. Both Jeffreys[11] and Pearson[12] indicate that since the time of Laplace and Gauss no self-respecting physicist ever gave any quantitative information without accompanying estimates of the accuracy of the observations. The assumption always was, however, that these variations were a function of either experimental defects or limitations of the measuring instruments being used. The immutability of these mechanical laws remained unchanged.

It is indeed remarkable that Pearson in 1892, three years before Roentgen discovered X rays, wrote:

> It is no discredit to the great structure of physical chemistry to assert that the absolute sameness of the molecule is only a statistical sameness, and that an ultimate individuality, a variation within the class may be hypothecated as a means of describing new developments which may hereafter be observed when the powers of discrimination are finer.[13]

The conviction of scientists like Aston that their measuring instruments were more refined than the residual error of the atomic weight measurements which they were deriving led to the discovery of isotopes. This constant search for explanations of the variation of phenomena in excess of what scientists felt was the residual error of measurement culminated finally in the indeterminacy principle of Heisenberg. It is this principle that has established the fundamental statistical nature of the universe, challenging the old deterministic view.

[11] Harold Jeffreys, *Theory of Probability* (London: Oxford University Press, 1939), p. 13.

[12] Karl Pearson, *Grammar of Science* (London: Black Publishing Co., 1911).

[13] *Ibid.*, p. 156.

The pioneer departure from the deterministic view of nature was made by Boltzmann when he formulated the kinetic theory of gases on statistical assumptions applied to macroscopic collections of gas molecules in order to explain the flow of heat from a low temperature gradient to a high temperature gradient.[14]

Heisenberg found that the position and the velocity of the smallest known physical particle cannot both be determinate. If either velocity or position is specified, the other is replaced by a probability distribution.

Now, although the mechanical view of nature has been replaced, can we conclude that its reign over the physical world was a misfortune in the same way that many theological concepts have restricted the development of science in the past? Quite the contrary. The mechanical theory never resisted investigation on the grounds that it was beyond the realm of scientific investigation. Its assumptions guided experimentation and the formulation of scientific thinking so that eventually enough information was gathered to replace it by a more satisfactory view of the universe that must now withstand a similar set of scientific tests. It is for this reason that scientific theorizing serves so useful a purpose.

The mechanical view of nature still remains a very useful concept for macroscopic physical phenomena. It still provides the foundation for mechanical and civil engineering applications of physical phenomena.

Every science strives to reorganize itself into a logical pattern on the model of the most successful systems in the past. Good examples are dynamics after Newton and Lagrange, thermodynamics after Fourier and Clausius.[15] It is these considerations as much as the economical convenience of operation that have led engineers to develop systems of standard data. It was an inward urge for pattern and order that has persisted as well as a more economical method of securing results. Thus begins the pattern of theoretical formulation of a time study system that

[14] Richard von Mises, *Probability, Statistics and Truth* (New York: Macmillan Co., 1939), p. 268.

[15] Cohen, *Reason and Nature*, p. 109.

logically explains variations in production rates from a minimum number of assumptions.

This attempt to fit the process of determining standards into a system is dictated by the principle of sufficient reason, of which causality is a special instance. Cohen states this principle as follows: "Everything is connected in definite ways with definite other things, so that its full nature is not revealed except by its position and relations within a system."[16]

The implications of this reasoning have been examined by Heidbreder in the field of psychology, a field of investigation closely allied to industrial engineering. In defending the right of psychologists to formulate systems, she writes:

> Why does psychology, which is trying so hard to be a science, build structures of speculative thought that it cannot hope for years to verify by established fact? Briefly because it has reached a place where such a procedure is all but inevitable. . . . It has been subjected to three of the most powerful stimulants to thought: increasing knowledge, grave concern, and persistent doubt.
>
> And so it is not surprising that psychology, which, according to its professed code, ought to settle down to the sober acquisition of fact, has indulged at times in riotous thinking and has produced systems of thought for which the factual proof is admittedly lacking and will be obtainable only in the remote future. For psychology is surrounded by the conditions of such thought. Its facts are numerous enough to be suggestive, not numerous enough to be conclusive . . .; its subject matter is close to the personal interests . . . which are peculiarly likely to arouse the concern of human beings.
>
> This means in turn that systems of psychology are to be regarded not as statements of scientific knowledge, but as tools by which scientific knowledge is produced; not as accounts of scientific fact, but as means of acquiring scientific fact. They are the scaffolding within which the structure of the science of psychology is being erected, as necessary as the scaffolding and as impermanent. . . .[17]

It is in this spirit that I propose a method of thinking about time study somewhat in the same tradition as Shewhart[18] developed for the field of quality control. Although I independ-

[16] *Ibid.*, p. 150.

[17] Edna Heidbreder, *Seven Psychologies* (New York: D. Appleton-Century Co., 1933), pp. 8–13.

[18] W. A. Shewhart, *Economic Control of the Quality of Manufactured Product* (New York: D. Van Nostrand Co., 1931).

ently conceived the possibilities of this method of applying probability reasoning to the field of time study, an investigation of the literature of the field discloses that Lorge[19] had suggested the possibilities of such an application to criteria establishing the workability of wage incentive payment plans in an address before the American Management Association. However, the approach suggested by Lorge differs substantially from the point of view which I take.

Before developing a time study system it would be well to draw certain conclusions from the analysis we have made of the applications and the nature of the scientific method.

It appears that the particular rules that apply to a set of phenomena depend very much upon the level of the collective at which we are describing what is happening.

The behavior of little Heisenberg particles is completely indeterminate in their ultra-microscopic world. In huge collections, however, they show macroscopic properties which have been systematized by our laws of mechanics. However, let us assume that we are tiny people living in a Heisenberg particle world; the laws of mechanics would be of extremely limited usefulness in guiding our everyday behavior. The accuracy and usefulness of these laws start at zero at the ultimate particle level and increase as the collective of particles increases, leveling off at a very high constant long before they reach a size large enough for us to be aware of in our workaday world.

What we are attempting to say is depicted graphically in Figure 1.

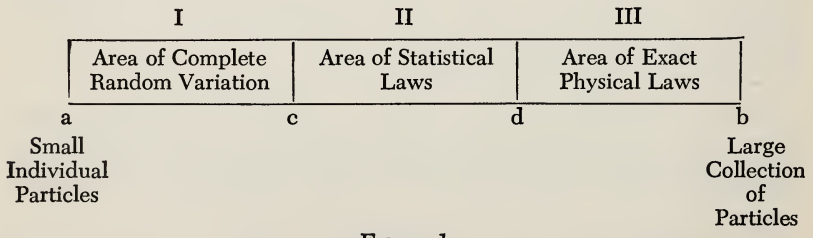


FIGURE 1

[19] H. W. Haggard and Irving Lorge, *A Physiologist and a Statistician Look at Wage Incentive Methods* (New York: American Management Association, 1937), p. 23.

Let line *ab* represent the variation in the size of a collective of Heisenberg particles. Then Area I will represent that area which is completely indeterminate, or, to put it another way, is subject to a variable chance cause system. Area II between *c* and *d* is the area in which statistical laws begin to apply. In this area we can say that a constant chance cause system is at work. Finally, the range *db* occupies the area of exact physical laws in which we generally say a single constant cause system is at work. Naturally, these areas are not tight compartments. They shade into one another and where one begins and the other ends is a moot question. However, Area III starts at so small a particle collective size in our physical world that we are seldom if ever aware of the existence of Areas I and II in the field of physics.

The ranges of measurement accuracy vary in fields different from physics. In biology and medicine our workaday world operates somewhere between Areas II and III. Thus, the major interest of famous statisticians like Fisher[20] and Tippett,[21] who draw most of their examples from biological fields, is in biology. In economics we seem to be on the borderline of Areas I and II. Hence, the sometimes amazing discrepancy between what a statistician predicts and what happens.

I would say that the most important task confronting an investigator attempting to establish a time study system would be to determine into approximately what area time study measurements are likely to fall. The question might be formulated somewhat as follows: Are industrial time studies taken within a variable chance cause system, a constant chance cause system, or a constant cause system?

What happens to any individual Heisenberg particle is of relatively little importance in our everyday world. What happens to individual human beings is exceedingly important. Hence, although we can be guided completely by the macro-

[20] R. A. Fisher, *Statistical Methods for Research Workers* (Edinburgh: Oliver & Boyd, 1938).

[21] L. H. C. Tippett, *The Methods of Statistics* (London: William & Norgate, 1941).

scopic laws in physics without any regard for what happens to any individual member of the physical collective, our freedom is somewhat more restricted in dealing with collectives in which the individual person is a member of the collective under consideration. Even if the size of the human collectives available were commensurate with those found in the submicroscopic physical world, this would still be true.

CHAPTER 4

Time Study as a Scientific System

If the foundation of any modern time study system is the principle of sufficient reason, then the keystone of its arch is the acceptance of the statistical view of nature. The view—that behind the variations in the measuring instrument was the basic variation in the phenomenon being measured itself—expressed itself in the conjecture of Charles Peirce, the American philosopher, long before its confirmation in the demonstration of the Indeterminacy Principle in physics.[1]

There may be objection at this point that we are doing exactly what we accused Taylor of doing: artificially transplanting this time the statistical view of physical reality, instead of the mechanical dogma, into the time study field. The time study field, which is made up of measurements in the relationship of men and machines, is perhaps more closely related to physiology and psychology than to engineering with its physical foundations.

In Chapter 3 we have examined the implications of the statistical view of the physical field. What is the status of the statistical view of nature in the biological field?

The status of the mechanistic view of biology has deteriorated completely. The failure of the attempt to reduce physics itself

[1] Morris R. Cohen, *Preface to Logic* (New York: Henry Holt & Co., 1944), p. 146.

to a mechanical picture naturally discredited Huxley's mechanistic version of the life structure. There is no reason to believe, however, that just as mechanics required new concepts, not derived from its primary assumptions, to explain electromagnetic phenomena, so life requires an explanation outside those derived from physics and chemistry.[2] We are thus left in a position where we can only hope to predict phenomena on the basis of the treatment of the variation of observations by the rules of statistical inference.

The problem of time study is the problem of reasoning from the time taken for a particular performance to the time taken for future execution. In this sense it presents the problem offered by the whole field of inductive reasoning.

It had been taken for granted that deduction and induction are two contrasting methods of reasoning about things. Deduction argued from the universal to the particular, induction from the particular to the universal. Actually, induction is in many cases disguised deduction. For example, I observe that at a specific time *A* can complete a job in x minutes. I, therefore, conclude that *A* should be expected to complete this job in x minutes in the future. Reducing this argument to its elementary syllogistic form, we find that we have tacitly assumed that *A*'s present performance and future performance belong to the same class:

1. The job time satisfactory for *A* at the present is satisfactory in the future;
2. Job time x is satisfactory for *A* today;
3. Hence, job time x is satisfactory for *A* tomorrow.

Thus what is apparently an inductive inference actually is a deductive inference from an assumption of our major premise.[3]

But it is the assumption of this major premise that is the meat of the investigation. It is this major premise which we seek to

[2] Morris R. Cohen, *Reason and Nature* (New York: Harcourt, Brace & Co., 1931), p. 242.

[3] *Ibid.*, pp. 115–16.

establish. The syllogism serves to emphasize the real condition. What distinguishes valid generalization from invalid generalization is the homogeneity of the class from which members have been examined.[4] The problem of establishing the homogeneity of populations and estimating their shapes and statistical parameters belong to the field of statistical inference which we shall have occasion to examine somewhat later. This is in reality a restatement of the question with which we ended Chapter 3: Are time studies the result of a variable chance cause system, a constant chance cause system, or a constant cause system?

Time study problems fall into three general classes of operations; those which are paced by an automatic machine, those whose speeds are controlled solely by the worker, and an intermediate group which are a combination of these two. They consist of a combination of machine paced elements and worker paced elements. Examples of machine paced operation are the jobs found on the assembly line in a typical automotive plant. Examples of worker paced operations are the sewing machine operations in a garment factory. Examples of combination operations are typical machine shop operations in which a set-up time is man paced and the cutting time is machine paced.

The development of a time study system can best begin with an analysis of the four main classes of variations influencing the quality performance and production rate of workers.

Professor Myers lists three classes of variations: the mechanical, the physiological, and the psychological.[5] To these we may add a fourth, the sociological. Each of these classes of variation, the mechanical, the physiological, the psychological, and the sociological, is in turn dependent on a wide number of specific subvariables. Let us term each major type of variation a parametric function of the production rate at which a group of workers labor to do a specified job. Each parametric function

[4] Morris R. Cohen and Ernest Nagel, *An Introduction to Logic and Scientific Method* (New York: Harcourt, Brace & Co., 1934), p. 277.

[5] Charles S. Myers, *Mind and Work* (New York: G. P. Putnam's Sons, 1921), p. v.

can in its turn be interpreted as a function of a wide number of subvariables.

If we assume the mechanical view of nature and postulate the all-seeing mind of Laplace that had discovered the interrelationships of all phenomena, we need but set up the form for the following functions and solve our equations:[6]

Thus let X = The time taken to perform an operation.

Let L_1 = The parametric function governing the mechanical sources of variation.

Let L_2 = The parametric function governing the physiological sources of variation.

Let L_3 = The parametric function governing the psychological sources of variation.

Let L_4 = The parametric function governing the sociological sources of variation.

(Equation 1) Then $X = f(L_1, L_2, L_3, L_4)$

$$L_1 = \phi(a_1, a_2, \ldots, a_n)$$

Typical mechanical a_i influencing the production standards would be the quality of the tools used and the consistency of materials.

$$L_2 = \pi(\beta_1, \beta_2, \ldots, \beta_n)$$

Typical physiological β_i would be the effects of workers' nutritional habits, sleeping habits, and basic muscular development.

$$L_3 = \mu(\gamma_1, \gamma_2, \ldots, \gamma_n)$$

Typical psychological γ_i would be aptitudes such as manual dexterity, reaction time, *et al.*

$$L_4 = \eta(\delta_1, \delta_2, \ldots, \delta_n)$$

Typical sociological δ_i would be the effects of relationships among workers and between worker and foremen.

[6] For earlier developments of the same ideas, see: William Gomberg, "This Union Helps Management Simplify Methods," [1944] *Proceedings of the National Time and Motion Study Clinic* (Chicago: Industrial Management Society, 1945), and William Gomberg, "The Union Looks at Management Engineering," [1945] *Proceedings of the National Time and Motion Study Clinic* (Chicago: Industrial Management Society, 1946).

It is at once apparent that these variables are not independent, but are mutually dependent. Sociological factors like the quality of the relationship between the workers and the management have their psychological effects. The psychological forces in turn have their physiological effects, as, for example, when emotional excitement is followed by a nervous stomach. In turn, physiological considerations such as poor nutrition affect the mental health of the working group. Effects and cross-effects like these can be multiplied endlessly and should give some idea of the complexity of the mathematical relationships that would have to be conceived by the Laplacian universal mind.

If time study is forced to abandon mathematics at this juncture, it is not only because the material with which we are dealing is vague and nonquantitative but also because mathematics has not yet developed the power to treat phenomena as complex as we are handling, even assuming a purely mechanical view of its nature.

It is fashionable to permit our enthusiasm for the new electronic computors to assume immediately that what mathematics couldn't handle before will now readily reveal itself. Unfortunately, the new electronic computors cannot handle variables for which we have not defined measurements as yet. This problem remains to be solved before we can even think of using the machine to minimize the complexity of the interaction of these variables.

Let us now change our point of view somewhat and substitute a statistical picture for the mechanical hypothesis. Then we may write:

$$\text{(Equation 2)}\quad Y = \int_{\beta}^{\alpha} f(X, L_1, L_2, L_3, L_4)\, dX$$

where Y represents the expected relative frequency that the time taken, X, to perform a unit of work will fall within the limits α, β, provided we are operating within an area of statistical control. An examination now of the nature of statistical

control should disclose the possibilities of developing a scientific time study system.[7]

The quality control engineers have defined the state of statistical control in terms of the famous bowl experiment. This bowl experiment has been used to postulate both the type of physical cause system that underlies a state of statistical control and the quantitative results that can be expected from a state of statistical control.

Shewhart describes an experiment in which a large number (N) of similar chips, on each of which a number is written, are put into a bowl. Selections are then made at random from the bowl, one at a time, in successive samples of size n. The chips are replaced and thoroughly mixed after each drawing. The results, when tabulated in the form of a frequency distribution, indicate that the differences between samples under such conditions are predictable from probability mathematics.[8]

Fortunately, we are able to make predictions about the behavior of statistically controlled systems without any knowledge of the detailed functional relationships which would be needed to solve Equation 1. All that we do need to know is that the phenomenon is subject to what has been postulated as a constant chance cause system.

In time study, once we should succeed in stabilizing the quantitative effects of the causes we have listed—the alpha's, beta's, gamma's, and delta's of the mechanical, physiological, psychological, and sociological classes of variation causes specified above, we would have a constant chance cause system similar to the system governing the bowl-drawing phenomenon. This is essentially what the quality control engineer attempts to do when, in producing units of the same product, he tries, as he puts it, to keep conditions *constant.*

[7] Note that we have adopted the Venn-von Mises' approach to probability as a relative frequency. For a full development of this concept of a probability collective, see Richard von Mises, *Probability, Statistics and Truth* (New York: Macmillan Co., 1939).

[8] W. A. Shewhart, *Economic Control of the Quality of Manufactured Product* (New York: D. Van Nostrand Co., 1931), p. 437.

We are not discussing the feasibility of the stabilization of this cause system by the time study engineer at the present time. The important thing to remember, however, is that unless this postulated condition can be realized or approximated in actual practice, it will be difficult, if at all possible, to predict any meaningful fixed production standard from a time study.

It is the constant system of chance causes that gives rise to frequency distributions, which follow statistical laws. The law of large numbers:

$$P\,[(M - Exc) > a] \leq \frac{\sigma^2}{na^2}$$

in which

P = probability or relative frequency
M = mean
Exc = a priori expectation
σ^2 = variance
n = number of readings
a = any preassigned number

gives a rational base to predictions made from a statistical population.[9] The only basis for inferring a production standard from a time study observation is the assumption that this time study constitutes a sample from a parent statistical population. Unless, therefore, we can demonstrate the existence of a state of statistical control, we have no right to infer any reasonably certain result from the time study except perhaps an empirical basis for mutual bargaining. The whole technique under such circumstances descends to the level of "guesstimating" like business forecasting.

It is important to remember, however, that we can conceive of a state of statistical control although we know of no way of obtaining such a state of control in practice. As Shewhart states: "The delineation of such a concept is a priori and definitive

[9] William Gomberg, "The Relationship between the Unions and Engineers," *Mechanical Engineering*, June, 1943, pp. 425–430.

whereas the application of the concept to a particular given physical state of control is hypothetical."[10]

Our ultimate objective is to be able to define a state of statistical control for a time study system in terms of a physical cause system and the quantitative characteristics of the sequence of observations produced by such a cause system.

It is quite apparent that before we can predict from any observations what representative time a job requires, the worker-machine system must be in a state of statistical control.

Thus, the problem of developing a sound time study system resolves itself into answering the following questions:

1. On the basis of an a priori examination of the physical cause system underlying production rates in a normal industrial environment, to what degree are we justified in assuming that these causes can be stabilized to an extent where we can postulate that all remaining variations are attributable to a constant chance cause system?
2. What are the quantitative criteria which time study can use in detecting the existence of such a state of control from a series of time study observations?
3. Can these statistical tools be used for purposes of approximation in industrial time study in the event it is found that a constant chance cause system does not characterize the industrial environment?

The answers to the first question very obviously lie in a detailed examination of all the sources of variation which we have detailed in Equations 1 and 2. This leads us into the field of engineering to determine the mechanical effects, to the field of physiology to determine the possibilities of stabilizing the physiological effects, and finally, to psychology and sociology for the effects attributable to these specialized fields. The answers to this question make up the subject matter of Chapters 6 through 9.

[10] W. A. Shewhart, *Statistical Method from the Viewpoint of Quality Control* (Washington, D. C.: Graduate School, Department of Agriculture, 1939), p. 11.

The answer to Question 2 calls for an examination of the field of mathematical statistics, of quality control as developed by Shewhart, and of some attempts made by Wiberg to apply statistical tools to methods engineering.[11] This survey makes up the balance of the present chapter.

The answer to the third question lies in determining to what extent the industrial environment can be brought under a constant chance cause system. It becomes again a matter of determining into just what area of Figure 1 (p. 48) the type of phenomena we are measuring fits. Are production rates predictable at all, and if so, with what percentage of accuracy? This will make up the subject matter of Chapters 6–9.

Let us for the sake of convenience postulate that it is possible to secure a constant chance cause environment in some especially well-managed factory. Mechanical equipment is perfect; materials are carefully inspected before being given to the workers. The workers lead normal, happy home lives in a small community where they are afforded excellent care, and they are completely unconcerned about the outside economic situation. Each person is responsible for his or her work, and worker motivation is no problem. We wish to go about setting production standards in such a factory. How shall we proceed? Each person is doing a different job so that there is no opportunity to compare performances. Again we emphasize that the jobs being performed are completely worker-paced. We are purposely excluding operations like textile spinning, which are completely machine-paced, and operations like operating punch presses, which have fixed machine cycles in the work pattern. The operations being performed in this hypothetical situation are very similar in type to those found in the needle industries where, with the exception of certain operations like button-holing on a cam machine, the operations are completely worker-paced. The sewing machine is nothing but a power-operated hand tool whose maximum speed of 5,000 r.p.m. is virtually never utilized

[11] Martin Wiberg, *The Work Time Distribution* (Chicago: McClure, Hadden & Ortman, Inc., 1947).

by the sewing machine operator. Her pace is fixed by her own working rhythm.

We have already pointed out that the whole problem of time study consists in our right to infer a general production standard from a sample time study. This is essentially the problem of estimating populations from sample studies. It is wise to recall again at this point that in this modern age of collective bargaining a time study, to be useful for rate-setting purposes, must have a residual error less than the usual increment or decrement to the hourly rate in any negotiations. Naturally it is presumed, among other ideal situations, that the factory is unionized.

The types of relationships that require study in rate setting in order to establish the existence of a statistically controlled universe of data are:

1. The internal relationships of the time study readings of a single person during a fixed interval of time;
2. The relationship of sample frequency distributions of a single person at different times during the working day;
3. The relationship of sample frequency distributions of a single person on different days;
4. The relationship of frequency distributions of different people on the same job.

We turn to the technique of statistical quality control. Each step in the quality control process (specification, production, and inspection) has its analogue in the formulation of production rates. When a product has passed the laboratory stage and is in the process of development for mass production, an expected price for labor is estimated, which in turn implies the specification of a production rate. Production is then undertaken to satisfy this cost, which in turn means meeting the expected production rate. Finally, there is observation of the results to see whether or not the production standard has been met.[12]

Thus, there is an equivalent in time study for each step in the

[12] Shewhart, *Statistical Method . . .*, p. 45.

quality control process. However, just as in quality control the three steps are part of a circular sequence, so the same is true in rate setting. In quality control, after a specification has been set, production is running, and inspection is taking place, statistical controls are used to refine the specification on the basis of inspections; so in time study the first expected standard is not necessarily kept fixed, but statistical controls may again be used to detect methods of increasing output.

The process may be described graphically as in Figure 2. The procedure turns around upon itself in an endless circle.

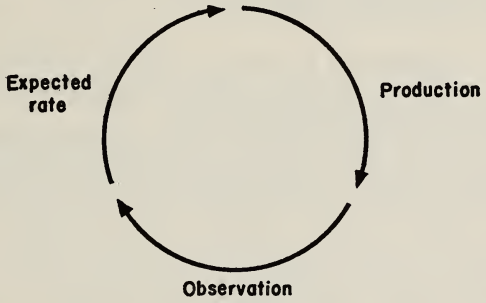


FIGURE 2

The techniques of statistical control that have been developed for quality control again may be extended to the rate-setting field where we have reason to suspect the existence of a constant chance cause system such as our ideal factory provided.

Shewhart's directions follow:

1. Specify in a general way how an observed sequence of n data is to be examined for clues as to the existence of assignable causes of variability.
2. Specify how the original data are to be taken and how they are to be broken up into subsamples upon the basis of human judgments about whether the conditions under which the data were taken were essentially the same or not.
3. Specify the criterion of control that is to be used, indicating what statistics are to be computed for each subsample and how these are to be used in computing action or control limits for each statistic for which the control criterion is to be constructed.

4. Specify the action that is to be taken when an observed statistic falls outside its control limits.

5. Specify the quantity of data that must be available and found to satisfy the criterion of control before the engineer is to act as though he had attained a state of statistical control.[13]

Each of these directions can be taken over bodily into time study thinking.

Wiberg approaches the same problem in a somewhat different manner which is reminiscent of the Pearson Curve System.[14] Wiberg arbitrarily assumes that the skew, range, and minimum reading of a frequency distribution are related to the fundamental traits influencing workers' performance. He describes his fundamental approach as follows:

> Under such conditions the performance by the worker is further analyzed as influenced by his aptitudes (native fitness), his habituation (practice effect), and his motivation (will to work).
>
> When work times are arranged into frequency distributions such analysis related the distribution characteristics to the following worker influences: Skew is related to motivation, width is related to habituation, and minimum time is related to aptitudes. The stronger the motivation and the habits and the more suitable the aptitudes of the workers are for the work, the greater is the skew, the smaller is the width, and the lower is the minimum time, respectively for the distribution.[15]

These, in any case, are the assumptions made. Rather arbitrarily defining what is an ideal distribution, Wiberg recommends comparison of existing distributions with the ideal distributions he postulates. If they do not match, then the operation requires attention. No quantitative means are assayed for comparison purposes except the graphs and captions reproduced on pages 64-65. Wiberg does warn the reader that the technique he proposes is to be confined to the laboratory for detecting methods of improvement. We may thus conclude that it will not be used for industrial rate setting. Its principal value seems to lie in the fact that this is the first time an effort has been made to apply the methods of statistical inference to time study analy-

[13] *Ibid.*, p. 25.

[14] W. Palin Elderton, *Frequency Curves and Correlation* (3d ed.; New York: Macmillan Co., 1938).

[15] Wiberg, *op. cit.*, p. 2.

sis on an inferential basis rather than merely as a descriptive attempt to summarize data.

The extension of quality control methods to the treatment of time study data might eventually lead to the reduction of all comparisons to the three traits developed by Wiberg. If it does, however, it will be as the result of a line of reasoning developed in a logical manner from the basic hypothesis of a constant chance cause system.

Statistical quality control became possible when the statistical measures that had been descriptive became tools for prediction with the development of sampling theory. The uses of these tools are based upon the fundamental assumption, as stated by Shewhart, that:

> . . . *approximate normality of an observed distribution arising under controlled conditions* may be taken as indicating that the cause system is in a state of maximum control. On the other hand, the fact that an observed distribution is not approximately normal is not sufficient evidence that the phenomenon is not in the state of maximum control.[16]

The rational basis behind this assumption is that the normal law is approximated as the contributory effect of each chance cause approaches equality and as the number of these causes approach infinity. However, it may readily be seen that it is possible to have a constant chance cause system with unequal but constant contributory effects. Under such circumstances the population of data would be statistically controlled, but the shape of the universe would differ from the Gaussian curve.

It was when this phenomenon was discovered, after many fruitless efforts to apply the normal law to every type of variation where a state of statistical control was suspected, that Karl Pearson developed his system of frequency curves.[17]

Since we have defined a state of statistical control as a balanced constant chance cause system, we can always expect fluctuations in sample time studies. However, these fluctuations

[16] Shewhart, *Economic Control . . .*, pp. 157–158.

[17] *Ibid.*, pp. 136–138, and L. H. C. Tippett, *The Methods of Statistics* (London: William & Norgate, 1941), pp. 60–61.

CHART 1: WORK-TIME DISTRIBUTION CHARACTERISTICS FOR WORKER FACTORS

SHOWING RELATIVE EFFECTS OF WORKER FACTORS UPON DISTRIBUTION OF 100 WORK-TIMES WHEN ENVIRONMENT AND METHOD-FACTORS ARE STABLE

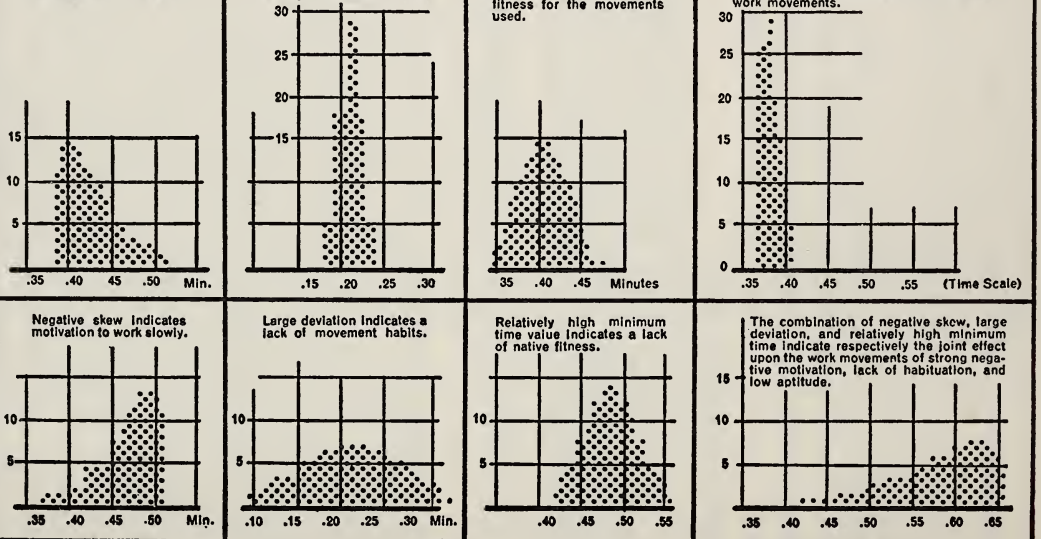
NOTE: Arbitrary values are shown on the time scales for illustration

KIND OF WORKER FACTOR
(Arbitrarily separated for purpose of illustrating the analysis)

COMBINATIONS OF JOINT EFFECTS
for two opposite and extreme cases.

TWO EXTREME CASES OF RELATIVE EFFICIENCY

	MOTIVATION	HABITUATION	APTITUDE	COMBINATIONS OF JOINT EFFECTS
WORKER B: High efficiency	Positive skew indicates motivation to move fast.	Small deviation denotes strong movement habits.	Relatively low minimum time value indicates high native fitness for the movements used.	The combination of positive skew, small deviation, and relatively low minimum time indicate respectively the joint effect of strong positive motivation, strong habituation, and high aptitude for the work movements.
WORKER A: Low efficiency	Negative skew indicates motivation to work slowly.	Large deviation indicates a lack of movement habits.	Relatively high minimum time value indicates a lack of native fitness.	The combination of negative skew, large deviation, and relatively high minimum time indicate respectively the joint effect upon the work movements of strong negative motivation, lack of habituation, and low aptitude.



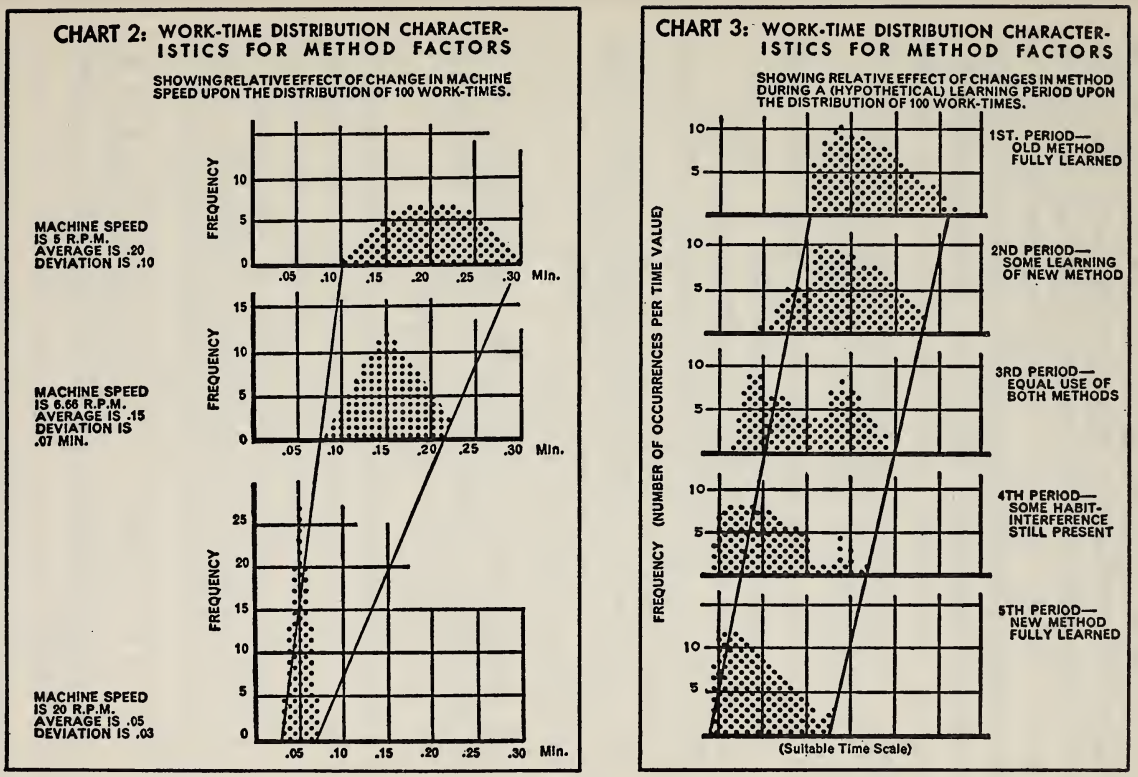


CHART 2: WORK-TIME DISTRIBUTION CHARACTERISTICS FOR METHOD FACTORS
SHOWING RELATIVE EFFECT OF CHANGE IN MACHINE SPEED UPON THE DISTRIBUTION OF 100 WORK-TIMES.
MACHINE SPEED IS 5 R.P.M. AVERAGE IS .20 DEVIATION IS .10
MACHINE SPEED IS 6.66 R.P.M. AVERAGE IS .15 DEVIATION IS .07 MIN.
MACHINE SPEED IS 20 R.P.M. AVERAGE IS .05 DEVIATION IS .03
FREQUENCY
.05 .10 .15 .20 .25 .30 Min.
CHART 3: WORK-TIME DISTRIBUTION CHARACTERISTICS FOR METHOD FACTORS
SHOWING RELATIVE EFFECT OF CHANGES IN METHOD DURING A (HYPOTHETICAL) LEARNING PERIOD UPON THE DISTRIBUTION OF 100 WORK-TIMES.
1ST. PERIOD— OLD METHOD FULLY LEARNED
2ND PERIOD— SOME LEARNING OF NEW METHOD
3RD PERIOD— EQUAL USE OF BOTH METHODS
4TH PERIOD— SOME HABIT-INTERFERENCE STILL PRESENT
5TH PERIOD— NEW METHOD FULLY LEARNED
FREQUENCY (NUMBER OF OCCURRENCES PER TIME VALUE)
(Suitable Time Scale)

FIGURE 3*

* Martin Wiberg, *The Work Time Distribution* (Chicago: McClure, Hadden & Ortman, Inc., 1947), reproduced by permission of the author.

should be predictable from probability theory. A further investigation of sampling theory discloses that the problem of sample interpretation depends upon the specification of the parent population and the size of the sample. Thus the problem of time study now becomes that of determining the parent population, the distribution of the sampling statistic within that population, and the size of the sample required to yield an accuracy at least commensurate with the collective bargaining increment or decrement to which we have previously referred.

Assuming that sampling fluctuations obey the law of large numbers, we now proceed to investigate their behavior in an effort to determine the answers to the following questions:

1. How shall we ascertain when the production rate of the man-machine system is statistically controlled?
2. How shall we establish the production-rate standard?
3. How shall we establish the limits of variation of the production-rate standard?
4. How large must a representative sample of the conceptualized infinite universe of continuous readings be?
5. How shall we select this sample?

These questions will be recognized as a translation of the directions specified for quality control on page 61 into the language of time study for rate-setting purposes.

The statistical tools required in order to infer the characteristics of our infinite parent body of future production rates from sample time studies made in our idealized factory are developed in the Appendix. It will be recalled that we have assumed a priori that there is reason to suspect the existence of a constant chance cause system, not necessarily, however, of a normal distribution.

A review of the questions we have just asked ourselves discloses that these statistical tools offer us the answers to Questions 1, 2, 3, and 4.

1. The use of the control charts in the homogeneity criterion

lends us a tool to test the statistical control state of our idealized man-machine system.

2. The production rate standard is fixed by the population arithmetic mean estimated from the sample means.

3. The limits of variation of the production rate standard are set by the population standard deviation estimated from the individual sample variances.

4. The number of readings of the particular study should be set so that three times the coefficient of variation is less than five per cent.

In answer to Question 5, the method of selecting a representative sample depends upon the definition of the universe we are conceptualizing; that is, which one of the four relationships outlined on page 60 we are interested in investigating.

The usefulness of each of these measures for rate-setting purposes, even under defined idealized conditions, is limited by the restrictions of the percentage collective bargaining increment, which we have arbitrarily defined as five per cent.

We now note that the theoretical techniques which we have developed thus far can be used to give a picture of all the four types of relationships. The control chart in Figure 16, shown in the Appendix, provides a technique for testing whether our time study observations come from a homogeneous population of data independent of the time, day, or week that the data was observed. Only one relationship remains untreated, that of frequency distributions of speeds of different people on the same job. The basic techniques could easily be extended to this relationship merely by redefining our universe to include a number of subjects and forming a population of the individual $\overline{X}p$'s. Using the statistical tools already described, we could attempt to predict within what limits the production speed of any newcomer to the job would fall.

However, the possibilities of the actual extension of these techniques to this fourth relationship are seldom possible because of an insufficient number of people to draw any valid conclusions

with any acceptable accuracy limits in practically all factories.

In addition, if the task of assuming a statistically controlled environment under idealized conditions for a single person is stretching credulity, introducing it for a group is going somewhat beyond the bounds of good sense. Inasmuch as our principal purpose consists in conceptualizing an ideal condition which we hope to approach asymptotically, it would serve little purpose to attach too much importance to this additional group concept. For this reason, the internal relationships between the speeds of various workers will be discussed later in our treatment of the practice of performance rating in Part Two.

In closing this discussion, it might again be emphasized that this analysis is not being proposed in the hope of finding immediate practical answers to the rate-setting problem. The principal purpose is to develop a rational approach that will provide a sound base upon which theoretical rate setting can rest. This theoretical base can serve as the center from which corrections for all deviations can be made, both in the idealized environment and in the industrial field.

Our approach to time study is very similar to the one taken by Whitehead in his study of the Relay Assembly Group at Western Electric. He stated his approach in the following language:

> . . . a population with important elements of homogeneity can be studied in two different ways. First, any one member can be examined and studied closely to obtain insight into its structures and its modes of functioning. This, in virtue of the homogeneity of the population, gives a basis of understanding to the observed behavior of any other member.[18]

At the present time we are studying the logical structure of the implications of time study in predicting the future behavior of a single individual, hence, our idealized factory. There will be time enough to examine variation between people when we discuss rating procedures.

Now that we have set our criteria of prediction and control

[18] T. N. Whitehead, *The Industrial Worker* (Cambridge: Harvard University Press, 1938), I, 4.

on the basis of a statistically controlled situation, let us proceed to an examination of the sources of variations, which, we suspect, interfere with our attempts to create a state of statistical control similar to what we postulate in the laboratory. It will be recalled that the sources of variations were divided into four classes: mechanical, physiological, psychological, and sociological.

Before proceeding to an examination of these external causes, however, it might be well, having postulated the criteria for limiting the accuracy of the man-machine system, to investigate the accuracy of the time measuring mechanism with which we are gauging this phenomenon.

CHAPTER 5

How Accurate a Time-Measuring Instrument Is Wanted?

Some years ago Abruzzi attempted to secure a picture of existing time study practices. He sent out questionnaires to 325 practitioners, teachers, and consultants and received 91 replies. The result thus can hardly give a conclusive picture of American time study practice. It is nevertheless interesting to review the results. One of the questions requested that the respondent list the time measuring instrument used. Forty-five reported that they used the stop watch 100 per cent of the time, ten that it was used 90 to 99 per cent of the time and two that it was used 85 per cent of the time. Wherever a designation of less than 85 per cent appeared, a substitute mechanism such as the motion picture camera or the wink counter or the marstochron[1] was used.[2] It is obvious that the stop watch has been considered adequate for the uses to which it has been put. How do these conclusions compare with other mechanisms when considering the relative effectiveness of the stop watch as a measuring instrument?

The principal problem in determining the accuracy of the

[1] A marstochron consists of an endless paper tape driven by a synchronous motor. Keys arranged like a typewriter are depressed at the reading point. The tape is read from calibrations on its surface. Elapsed times are thus recorded.

[2] Adam Abruzzi, "A survey of American Time Study Practice," *Time and Motion Study*, Vol. 2, No. 1, January 1953, London, pp. 11–25.

measuring instrument, where the variance of the data is important in deciding whether a state of statistical control exists, is to obtain not only a true average for a set of readings but as true a reading for each observation as can be obtained. In reviewing the data, the variance, as we have seen, is of extreme importance. Therefore, if variance of the measuring instrument is of the same order as the variance of the data being measured, we shall be in a state of hopeless confusion.

The stop watch, which was the first time-measuring instrument introduced into time study work, has been the subject of repeated controversy since the Gilbreths launched their attack against its use in a paper delivered before the New York chapter of the Taylor Society, December 16, 1920.[3]

Rereading the paper today, one cannot help feeling that they used the stop watch as a symbol of extensive rate-setting abuses upon which they were interested in concentrating attention. However, despite this consideration, some of the things that they had to say about the watch on purely a priori considerations stand up surprisingly well today on the basis of actual experimental evidence.

At the time, Harlow Person called for actual statistical investigation of the Gilbreths' claims that the average of a series of readings will not be a "true" one, despite the factor of compensating errors and an increase in the sample size by taking large numbers of readings.[4] It was only comparatively recently that this matter was investigated.

The rest of the attack can be understood only in the context of the particular situation within which the statements were made. The Gilbreths were leading an attack upon sloppy time study practice, especially the widely prevalent habit of neglecting adequate recorded descriptions of methods of performance. They were interested in promoting the intensive use of their more detailed motion-study techniques to standardize opera-

[3] "Symposium on Stopwatch Time Study," *Bulletin of the Taylor Society*, June, 1921, pp. 91–135.

[4] *Ibid.*, p. 98.

tions before measuring performance. The stop watch was attacked not so much as a poor measuring instrument but as the tribal symbol of a wide body of time study practitioners who were concealing guesswork under a time study guise. The use of the stop watch lent itself to this kind of abuse much more readily than did the careful techniques worked out by the Gilbreths; hence, the violence of their attack.

Along with this sloppy practice went the usual errors of extending observed numbers of four significant figures to as many as seven significant figures during computation. Errors of this nature can hardly be attributed to the stop watch. They can be attributed only to the ignorance of the user of the stop watch.

The only careful a priori analysis of the stop watch as a time-measuring instrument in time study work, which has appeared in the literature since the Gilbreths' day, is the study by Presgrave.[5] Presgrave's analysis is closely reasoned but, like so many other rational systems of thought which depend upon a priori reasoning, remains unsupported by observed data.

Presgrave discusses the accuracy of the stop watch both as to accuracy of readings and, then, as to method of reading and recording the data of an elemental time study. Discussion is confined to the watch graduated in hundredths of a minute.

Assuming that the average element is of .10 minute duration, Presgrave further assumes that this actually means that the true reading is somewhere between .095 and .105 minutes.[6] This would lead to an average possible error of about .01 in the average element of .10 minute duration. Practically the complete tolerance permitted by the collective bargaining increment or decrement has been used up in the most objective part of the time study—hardly a satisfactory state of affairs. Presgrave goes on to point out that in measuring therblig times with a wink counter, errors of a similar percentage magnitude are encountered.[7] This argument, however, is of only limited validity be-

[5] Ralph Presgrave, *The Dynamics of Time Study* (2d ed., New York: McGraw-Hill Book Co., 1945).

[6] *Ibid.*, p. 39.

[7] *Ibid.*, pp. 39–40.

cause the wink counter can be used on longer elements than therbligs and lend a much higher order of accuracy to readings than can the stop watch.

The use of the kymograph[8] can be dismissed completely since it is usable only under laboratory conditions and on a very limited variety of operations. The only extensive claims for the kymograph as a measuring instrument for foundational times in an industrial time study system are those made by A. B. Segur in an exchange of correspondence with the writer in 1943. Inasmuch as the details of the use of the mechanism and the results derived are treated as a business secret by the Segur organization, they cannot be judged until such time as they are published for critical evaluation by the scientific community. Segur's system, known as motion-time analysis, will be discussed when we evaluate present practices of setting standards from standard data.

Richard E. Leng made the pioneer study of the comparative accuracy of the decimal minute stop watch, the marstochron, and the Veeder Root counter. His material still retains much of its original value.

The interpretations and conclusions are my own; the data is Leng's. This analysis was first developed in 1943.[9] The particular part of the experiment from which I drew my conclusions was designed by Mr. Leng as follows:

A calibrated synchronous motor was set up to drive an endless loop of film. At fixed intervals a sticker was pasted over the frame of the film. Readings were taken on the stop watch, the marstochron, and the wink counter as the pasted frames bypassed a fixed pointer. In this manner observers could measure a calibrated time interval with definitely fixed end points which were much more exact than the arbitrarily chosen elemental

[8] The kymograph is an unwieldy apparatus. It consists of an endless belt of paper driven by a synchronous motor at a much higher speed than the marstochron. Industrial operations are designed so that at the end point of an element a photo-electric cell is actuated, leaving an impression on the paper.

[9] William Gomberg, "The Relationship between the Unions and the Engineers," *Mechanical Engineering*, June, 1943, pp. 425–430.

end point which the average time study engineer is compelled to choose in actual industrial operations. The observers were nine men whose time study experience varied from six months to ten years. Their experience was confined to making cumulative readings with the stop watch, none of them ever having used the wink counter or marstochron. The observers were interested enough in their professional work to be graduate students in advanced time study, working under Professor Robert K. Morrow of the New York University engineering faculty. Their competence was vouched for by him. Observers were switched from instrument to instrument to make sure that memorization of the pattern of time allocations did not interfere with the accuracy of their observations. Twenty readings were recorded for each observer, making a total of 180 readings on each instrument. (The analysis we are making omits consideration of the wink counter because it is impractical for time study work, if for no other reason, because of the noise that it makes in operation. Its principal usefulness is in motion and methods analysis.) The observations were tested for significant differences, though the marstochron readings indicated that the universe of data could be considered homogeneous for all the men. However, there remained some suspicion that the population of stop-watch readings did not constitute a homogeneous universe for the sake of analysis. We may assume, however, that these stop-watch readings do constitute such a group, as will be apparent later.

Table I is made up of a comparison of the stop-watch readings with the marstochron readings. Where less than 180 readings are recorded, the observer missed some readings during the course of observation. The analysis follows.

It will be noted from the table that the deviation of the observed arithmetic mean from the true reading varies from a low of 3.30 per cent for readings of .10 minutes to a high of 12.6 per cent for a reading of .025 minutes. When we realize that the permissible percentage error for our entire time study is plus or minus five per cent, it seems that this is indeed a large per-

TABLE I

		A	B	C	D	E	F
	Number of readings recorded	Known time lapse from the synchronous motor	Arithmetic mean	Standard deviation	Coefficient of variation $\left(\frac{C}{B} \times 100\right)$	Three times the coefficient of variation	Percentage deviation from the true mean $\left(\frac{A-B}{A} \times 100\right)$
Stop watch	172	6.25	6.045	0.926	15.3%	45.9%	3.3%
Marstochron	174	6.25	6.230	0.156	2.5%	7.5%	0.3%
Stop watch	163	2.50	2.816	0.836	29.7%	89.1%	—12.6%
Marstochron	174	2.50	2.443	0.165	6.8%	20.3%	2.3%
Stop watch	164	8.75	8.386	1.293	15.5%	46.4%	4.5%
Marstochron	174	8.75	8.777	0.171	2.0%	6.1%	— .3%
Stop watch	172	10.00	9.674	1.078	11.1%	33.4%	3.5%
Marstochron	173	10.00	10.030	0.170	1.7%	5.1%	— .3%

centage to throw away on the most objective part of the time study. In contrast, the marstochron indicates a range of deviation of 30 per cent from the true readings for readings of .10 minutes to 2.34 per cent from the true reading for readings of .25 minutes. This represents a decided improvement over the accuracy of the stop watch.

However, even more important than the percentage deviation from the true readings is the coefficient of variation for each of these samples. If we assume a normal population of data, indeed an optimistic assumption for our stop-watch readings, then plus or minus three times the coefficient of variation will indicate the possible deviation of any individual reading from the true value in terms of percentage of the arithmetic mean in 997 cases out of 1,000. This is particularly important because the lengths of the elemental time reading that we have chosen to study are those encountered in industrial practice every day. Thus the stop-watch readings indicate a tripled coefficient of variation varying from a high of 89.1 per cent for a true mean of .025 minutes to a low of 33.4 per cent for .10 minutes. Quite

obviously the variance of time study distributions becomes meaningless for any analysis whatever when measured by a measuring instrument with so large a residual error.

The marstochron, on the other hand, indicates a tripled coefficient of variation that varies from a high of 20.3 per cent for a true reading of .025 minutes to a low of 5.1 per cent for a true reading of .10 minutes.

Such circumstances would indicate that elemental time study with a stop watch which records elemental readings even as high as .10 minutes is completely unreliable for rate-setting purposes and should be replaced by a time study taken with the marstochron.

Abruzzi has taken exception to these conclusions as well as those proposed by Leng, himself. He points out that much of the variance of the time study data arises because the readings of the different stop-watch observers were pooled into a single distribution. He acknowledges that Leng pointed out that the pooling of these readings did indicate that the stop-watch readings were not in a state of statistical control. Abruzzi argues that if each stop-watch observer's readings were compared individually with the same observer's marstochron and wink counter readings, the stop watch would not show up as unfavorably as Leng's data indicated.[10] This is a valid conclusion only if we assume that every plant is going to have no more than a single stop-watch observer. The fact remains that a time study department should be free to pool observations freely. It can do so if these readings are made on the marstochron. The only question that remains is whether or not the relative inconvenience of using the marstochron imposes so heavy an economic burden on the process that the substitution is not feasible.

Another problem that arises in connection with the stop watch is whether snap-back readings or continuous readings should be preferred. In view of the demonstrated limited accuracy of the stop watch, it seems unsafe to trust snap-back

[10] Adam Abruzzi, "Effectiveness of Time Study Measurement Methods," *Time and Motion Study*, Vol. 2, No. 7, London, July 1953, p. 9.

readings without any over-all time check. It has been the policy for the Management Engineering Department of the International Ladies' Garment Workers' Union to insist that all time studies submitted to it by members of the management in dispute cases be performed by the continuous method. It has been found that this generally assists a skilled observer in tracking down the root of a technical complaint.

Lowry, Maynard, and Stegemerten find that, as a result of a reaction time of the observer and the momentary halting of the watch as it is snapped back to zero, an error of anywhere between two per cent and nine per cent is introduced into the time study from this source, depending upon the length of the cycle.[11] They definitely express a preference for the continuous method, as does Barnes.[12] Likewise, Holmes[13] and Steele[14] favor the continuous method. Professor Rogers found that certain individual time study men using the snap-back method accumulated errors as high as 20 per cent because of anticipating ratings. He is doubtful that any observer, no matter how experienced, can avoid an error of less than five per cent.[15] The *Production Handbook* recommends the use of continuous time in preference to snap-back methods.[16] It is interesting to note that Lichtner, one of the earliest pioneers in time study, in discussing the relative merits of snap-back readings versus continuous readings, touched upon a concept that seems to have been overlooked by most advocates of standard data, when he wrote:

[11] Stewart M. Lowry, Harold B. Maynard, and G. J. Stegemerten, *Time and Motion Study* (3d ed.; New York: McGraw-Hill Book Co., 1940), pp. 191–92.

[12] Ralph M. Barnes, *Motion and Time Study* (2d ed.; New York: John Wiley & Sons, 1940), p. 262.

[13] Walter G. Holmes, *Applied Time and Motion Study* (New York: The Ronald Press Co., 1938), p. 144.

[14] H. H. Steel, "Time and Motion Study Engineering" (Methods Engineering Associates, 1940), p. 5 (mimeographed).

[15] H. Barrett Rogers, "Making the Stop Watch Observation," [1941] *Proceedings of the National Time and Motion Study Clinic* (Chicago: Industrial Management Society, 1941), p. 13.

[16] L. P. Alford and John R. Bangs, *Production Handbook* (New York: The Ronald Press Co., 1944), p. 473.

The conclusions drawn from it [snap-back readings], however, are unsatisfactory because one element should not be abstracted and timed apart by itself. The time taken to perform one element is more or less related to the time taken for the preceding element, since the employee usually swings from one motion to another without the slightest hitch or stop. The repetitive relation disregards this relation.[17]

This represents a distinct departure from Taylor's attempt to subdivide all operations into elements that could be recombined into any pattern whatsoever. We shall have occasion to go into Lichtner's concept more fully when we examine present methods of developing rates.

More recent writers, however, indicate a trend to snap-back reading methods. It is interesting to consider the reasons they advance for this preference. Shumard lists the following reasons for his objections to continuous timing:

[Continuous timings include] odd movements or delays that are foreign and will not be allowed in the final analysis. . . . In an effort to show his own efficiency, the T.S.M. [time study man] will become so intent in filling in the windows [time-study blank spaces], instead of watching the job in detail, that inefficiencies in the operation before him escape his notice. In case he is suddenly interrupted by the foreman or others, he may go so far as to "fake in" certain time data that he has inadvertently missed. . . . Many operators do not perform their cycles exactly alike each time. Sometimes they transpose elements because it facilitates their work, or possibly on account of the fact that they are being timed. . . . In the continuous method the T.S.M. must, regardless of how long the operator is away from his job, record the periods of time the operator stopped to obtain a drink of water, or other stoppages for which a time limit taken from standardized data will be given.[18]

He finally concludes:

Should that study be filled with many items that were stricken out or radically adjusted, the attack of officials on the validity of the proposed time allowance might center around the items that were stricken out.[19]

Why not go the whole way and advocate secret time study? Recalling that time study can be justified on a scientific basis

[17] William O. Lichtner, *Time Study and Job Analysis* (New York: The Ronald Press Co., 1921), p. 168.

[18] F. W. Shumard, *A Primer of Time Study* (New York: McGraw-Hill Book Co., 1940), pp. 35–36.

[19] *Ibid.*, pp. 36–37.

only if it is considered as a sample of a larger universe of data, the sole reason listed here that deserves any attention is the objection that many observers transpose the order of elemental sequences from operation to operation. The way to solve this problem is not to circumvent it, but to investigate the validity of the underlying assumption that the best way for an operator to work is by never varying the cycle sequence. It may very well be that an investigation of the latest work in psychology and physiology will produce ample justification for varying the cycle sequence. It would then be necessary to conceptualize a completely new universe based upon a mixture of methods. This would in turn call for a mixture of observations. Of course, this represents a departure from such directions for the use of the snap-back method of time study as, "In case the operator . . . performs movements that are not to be considered as official movements, the watch is stopped."[20]

Shumard's persistent resort to standard data for all foreign elements, like taking a drink of water, is a concept that will be examined in the discussion of physiological variables.

Carroll justifies what he calls the small error in each observation under the snap-back method of time study by pointing out, "This is inconsequential in comparison with the probable errors in human judgment included when rating the performance."[21] No objection can be interposed except to observe that this very sound advice is somewhat at variance with his admonition on union participation in time study:

> At the same time, it is not to be assumed that union-trained time study men should act jointly with the duly constituted standards setters. That would presuppose the determination of incentive standards through negotiations or compromise. Such a procedure is wrong.[22]

The best defense of snap-back methods of recording time studies as against the continuous method is given by Presgrave.

[20] *Ibid.*, p. 37.

[21] Phil Carroll, Jr., *Timestudy for Cost Control* (2d ed.; New York: McGraw-Hill Book Co., 1943), p. 70.

[22] Phil Carroll, Jr., "Better Wage Incentives," *Advanced Management* (April, 1945), pp. 60–63.

His preference for snap-back watch timing as against continuous timing is best understood in the light of his basic approach to the time study concept. In describing the function of the time study man he warns:

> He must not be held to a slavish adherence to method such as is demanded by continuous reading, but should be as free as possible to observe and to speculate. If, in doing so, he fails to secure an over-all time, or misses an occasional element time, it may well be considered that these are venial errors which, nine times out of ten, need have absolutely no effect on the final result.[23]

> As a study builds up, various phenomena may show themselves. There may be occasional element times much higher or much lower than the general run. There may be an unexpected uniformity in certain element times. Some pattern or sequence may show itself. Any of these may shape the time-study man's judgment, may lead to profitable speculation, or may force on his attention certain deviations from the norm that he would not otherwise have noticed.[24]

It seems that there has been a confusion here between the function of a stop watch as a tool in methods study and its function as a measuring instrument in time study for rate-setting purposes. Before a time study is taken, we presuppose the existence of a homogeneous universe of which the time study is viewed as a sample. If this concept is abandoned and the time study observer is permitted full sway of his speculative judgment in accepting and rejecting readings, then the whole scientific basis for time study as an objective measurement disappears; it becomes a function of the intuitive sense of the time study man. The only acceptable criterion for judging the relative accuracy of the methods of reading the stop watch is whether snap-back or continuous timing gives us a more objective measure of what is happening. That is the method that reduces the element of individual human judgment to the minimum and should be the guiding criterion.

The element of human judgment and its role in time study will be treated further in our discussion of current industrial methods of analyzing data.

[23] Presgrave, *op. cit.*, p. 43.
[24] *Ibid.*, pp. 43–44.

Presgrave goes on to recommend the snap-back method as a means of discovering whether or not an operator is stalling. This is something that should be determined before taking a time study rather than during the process.

Finally, in discussing time study machines such as the marstochron as a substitute for the stop watch, he concludes that they provide an unutilizable degree of accuracy.

This degree of accuracy, however, does become important when we are interested in the variance of the distribution of our time study observations, particularly when we wish to distinguish between the variance of the subject and the variance of the observer. We have treated this amply in the preceding section.

Under the circumstances, we may conclude that the continuous method of timing offers a method of objective measurement superior to the snap-back method.

Later studies by Lazarus[25] and Lehrer[26] open the question of the superiority of the continuous method of timing over the snap-back method of timing. Lazarus designed an apparatus which he calls a stimulus generator. The apparatus consisted of a synchronous motor to drive a reel of tape. The motor was connected with a variable-control speed system. Time intervals were measured off by perforating the tape at fixed distances. A source of light was placed in the field, and whenever a tape perforation passed the light source, the beam of light passed through the opaque tape and actuated a photoelectric cell. The photoelectric cell in turn actuated an auditory buzz signal. Another light was introduced into the mechanism so that a visual signal was available as well.

The Lazarus test was given to a total of 50 industrial engineers representing five large companies from the states of Illinois and Indiana. A total of five thousand times were registered. They consisted of 100 readings for each observer, half of which

[25] Irwin P. Lazarus, "Nature of Stop Watch Time Study Errors," *Advanced Management*, Vol. 15, No. 5, May 1950, p. 15.

[26] J. W. Lehrer, "How Accurate Is the Stop Watch?" *Journal of Industrial Engineering*, Vol. III, No. 2, Georgia, August 1952, p. 6.

depended on an auditory end point and the other half on a visual end point. The individual readings ranged from 4–39 hundredths of a minute. Each observer used either the snap-back or the continuous method in accordance with his past practice. "These errors made by the engineers were determined by finding the difference between the engineers' element times and the actual element times (computed using the tape velocities and the distance between holes). If an element time was greater than the actual time, the error was considered positive; if an element was less than the actual time, the error was considered negative."[27]

The results showed that the standard deviations of the errors in both the snap-back and continuous methods of timing were equal to 0.081 minutes. The data also indicated that the mean error of the errors in the continuous type of readings equaled +.000097 minutes whereas those of the snap back equaled —.0008 minutes.

Lazarus goes on to observe that "The magnitude of the error is independent of the size of the time elements. . . ."

He finally concludes that, in the hands of a competent experienced industrial engineer, both the snap-back and the continuous methods of watch readings yield equally good results for all practical purposes.

Lehrer accepts these results and conclusions completely and uses them to build a new statistical procedure for time study in which he uses the standard deviation of 0.008 minutes to compute the number of readings to minimize the effect of the variation of the timing instrument on the over-all chance variance of the time study.[28]

Abruzzi, on the other hand, rejects the method of snap-back readings. His rejection is conditioned, however, by his suspicion that the arbitrary division of the work cycle into elements is unwarranted. He refers to the warning of Lichtner, to which

[27] Lazarus, *op. cit.*, p. 16.

[28] J. W. Lehrer and R. Modev, "Statistics in Time Study," *Journal of Industrial Engineering*, Vol. IV, No. 1, February 1953, p. 9.

we have referred earlier, and advances some evidence of his own. Although he also found that the standard deviations for the cycles and the elements into which they were divided were of comparable magnitude under both the snap-back and continuous methods, other considerations led to his dismissal of the snap-back method. For example, one of his tests showed that the corresponding average element and cycle times were found to be significantly different although the differences between the average cycle times were less distinct than the difference between the element times.[29]

Inasmuch as all observers seem to indicate that the snap-back method leads uniformly to a lower estimate for element times than the continuous method, the continuous method continues to justify itself, if for no other reason than that of an over-all check whereby the complete elapsed cycle time is matched against the sum of the elements.

It should be noted, however, that these objections to the use of the watch for snap-back purposes arise from a fundamentally different opinion about the validity of any elemental breakdown rather than the fundamental nature of the timing method. In a sense, however, the two concepts cannot be separated. This problem of the elemental breakdown of the operation will be further discussed in our chapters on Defining the Job and Standard Data.

Furthermore, inasmuch as we are attempting to study the possible homogeneity of time study data under a constant physical chance cause system, the marstochron offers an even more accurate way of measuring time. The elimination of the variation in the observer's judgment in judging the variance of the time study distribution justifies the small additional trouble which using the marstochron, even under industrial conditions, entails.

Thus the establishment of the accuracy limitations of the

[29] Adam Abruzzi, "The Snap Back and Continuous Methods of Taking Time Studies," *Advanced Management*, Vol. 16, No. 4, April 1951, p. 11, and *Work Measurement* (New York: Columbia University Press, 1952), pp. 227–245.

time-measuring device leads to the examination of the effects of these sources of variation that, on an a priori basis, are likely to disturb the physical chance cause system. Before we go on, it might be wise to review our development thus far.

It will be recalled that ultimately we are interested in discovering into what classification of measurable data, as shown on Figure 1, page 48, time study phenomena fall.

We have abandoned any attempt to find the answer in the realm of exact physical laws. In an attempt to discover whether the answer lies in the field of statistical measurement, we have postulated a theoretical time study structure based upon a constant physical chance cause system. We know that unless we can relate time study phenomena to some aspect of such a system, unless we can show that time study can be organized into some sort of homogeneous universe of data, then the major premise of the syllogism on page 00 cannot be established and prediction of future production rates becomes that much more questionable.

In postulating a theoretical time study universe governed by a constant chance cause system, we have developed tools and methods in Chapter 4, indicating that the most useful individual statistics in predicting future performance within such a system are as follows: 1. the population arithmetic mean; 2. the variance; 3. the combination of the arithmetic mean and the variance into the coefficient of variation. These yield measures of the applicability of the basic data within a defined margin of error.

We have established the relative accuracy of our measuring tools, the stop watch and the marstochron. We now proceed to an analysis of the possible sources of variation that can interfere with the constant physical cause system. Our object in this investigation is threefold:

1. To estimate, if possible, what their effects are;
2. To know to what extent they can be controlled;

3. To know, in the event that they cannot be controlled, to what extent and to what limits of accuracy we may correct them for their effect.

We now recall the four classes of variation previously discussed: the mechanical, the physiological, the psychological, and the sociological.

It is time to analyze in some detail the effects of these classes of variables. The following chapter will be devoted to a discussion of the effects of the first of these, the mechanical, on the constant physical chance cause system we have postulated.

CHAPTER 6

Mechanical Sources of Variation

Mechanical influences such as the condition of the tools and machines present virtually no problem under ordinary circumstances. Either they can be standardized and their effect made a constant in the chance cause system, or proper allowances can be made for their determination. This was true before the war. It was not true either during the war or in the period immediately after its conclusion because of material shortages. In fact, a review of the cases handled by the Management Engineering Department of the International Ladies' Garment Workers' Union disclosed that there were mechanical difficulties in something like 60 per cent of 420 rate complaints during that period.

The ability of manufacturers to set specifications on the type of materials they would accept had been suspended. Their willingness to accept any raw or semifinished material at all seriously disturbed rate structures over the country. The rates of the knitting machine operatives of one large knitting mill in Pennsylvania were constantly being adjusted in an effort to overcome the variation in poundage output of operatives because of variation in the number of slubs in the cotton yarn. This was a common pattern throughout the whole country.

Similarly, cutting tools no longer held their edge the way they once did. Alloy steels were not readily available and the inability of management to predict the behavior of these unknown

materials was delaying standardization of their effects. Under the circumstances, we may conclude that this type of mechanical effect upon the chance cause system can be neglected, that is, to the extent that we are attempting to determine which variables tend, by their very nature, to destroy the constant cause system. The manner in which allowances traceable to this source are treated will be examined when we inspect present industrial time study methods of analysis.

Other mechanical sources of variation seem to lie somewhere in between a purely mechanical effect upon the constant chance cause system and a physiological effect in that they have generally been interpreted to affect the fatigue rate, a concept that we are yet to examine. Whitehead,[1] Roethlisberger,[2] and Mayo[3] of Harvard have studied the effect of a great many mechanical variables upon the daily productivity of operators. The Western Electric experiment which they interpreted has received its share of plaudits from the industrial engineering profession, but nowhere have I seen any attempt to interpret any of its *results in the light of rate setting.*

It will be recalled that the Western Electric researches had their inception in a number of experiments to determine the effects of variation in illumination on output rate.[4]

Logically enough, the initial increase in illumination was accompanied by significant increases in output. The girls being studied were aware of the fact that they were participants in an experiment. However, a subsequent reduction in the illumination intensity failed to elicit a corresponding downward trend in production. The finding that the output was independent of the amount of illumination led to the suspicion that the assumptions about mechanical conditions, which had hitherto

[1] T. N. Whitehead, *The Industrial Worker* (Cambridge: Harvard University Press, 1938).

[2] F. J. Roethlisberger and William J. Dickson, *Management and the Worker* (Cambridge: Harvard University Press, 1939).

[3] Elton Mayo, *The Human Problems of an Industrial Civilization* (Boston: Division of Research, Graduate School of Business Administration, Harvard University, 1946).

[4] Roethlisberger and Dickson, *op. cit.*, pp. 14–17.

governed the thinking of engineers in determining output, were faulty.

The superficial interpretation of the results of the illumination experiment would be that the constant chance cause system, which we have postulated as the foundation of a logical time study system, is independent of this variable. Yet illumination is one of a class of mechanical-physiological variables that one would think, a priori, should affect output. Hence, if an investigation of all of these variables should indicate that their variations are independent of output, then the idealized factory condition could be extended to the industrial environment, and time study for rate-setting purposes could be interpreted purely as a mathematical-statistical technique. All that would then be necessary would be the development of the best mechanical motion pattern, standardization of the job, and a statistical analysis of the time required to perform the work. We shall have occasion in a later chapter to investigate the assumption behind the "one best method" in our study of psychological sources of variation of work rate. The rest of this chapter will be devoted to the mechanical sources of physiological variation.

Whitehead attempted such an analysis of similar physical variables among others in his monumental statistical analysis of the relay assembly group of workers in the Western Electric experiment.[5]

Among the factors of the mechanical material environment that were investigated by Whitehead, in an attempt to determine their effect on output, were:

1. Changing the type of relay upon which a worker was working;
2. Temperature and relative humidity;
3. Hours of rest of the workers themselves;
4. The effects of periodical illness on women workers.

The experiments were conducted over a period of six years, from 1927 to 1933. The five workers involved knew the purpose

[5] Whitehead, *op. cit.*

of the experiment. They were paid on a group wage incentive payment plan. It is important to remember, however, that, whereas in the regular factory the number of those participating in the group wage incentive payment plan was 100, in the test room it was made up of only six. Another limitation on the accuracy of the conclusions drawn was that production rates were being measured in terms of standard relays. Inasmuch as each girl was compelled to work on different styles of relays at different times, the technical department of Western Electric worked out a conversion factor for each relay so that it could be interpreted in terms of standard relays. Since we are studying methods of fixing production standards, drawing inferences about relative production tasks from such a study might lead to circular conclusions. However, the experiment took place over so many years that it is safe to accept the basic trends as representative, on the assumption that the pattern of samples of relays remained constant over a long period of time.

During the period of this experiment, working conditions, such as rest periods and working hours, were changed and restored. Irrespective of what was done, whether or not rest periods were added or taken away, working hours shortened or extended, individual production figures kept climbing consistently from an average of something in the neighborhood of 50 standard relays per hour until it reached a figure somewhat in between 70 and 80 standard relays per hour five years later, depending upon which operator was being observed. It was in an effort to explain these variations by a number of hypotheses that Whitehead undertook his elaborate statistical study of the experimental observations. This increase was particularly surprising in that each of the subjects had had a long term of training in assembling these relays and, on the basis of what is known about learning curves, had reached the level learning plateaus.

Within the limitations we have mentioned, Whitehead drew the following conclusions:

> Taking all this evidence together, we can conclude that changing the

type of relay has no effect on the immediate rate of output in the case of the experts, and a slight adverse effect with the novices.[6]

The effect of changes in temperature and in relative humidity on the output rate of the operators in the Relay Test Room has been found to be negligible in all cases, as compared with the changes of output rate that were constantly being experienced. This applies both to variations in outside weather and to variations in room conditions; and it holds in each case, whether we consider seasonal, weekly, or daily variations.[7]

Daily rates of work (trends removed) failed to correlate with hours of rest on the second nights previously. . . . Daily rates of work (trends removed) sometimes showed a weak and doubtfully significant correlation with hours of rest on the previous nights.[8]

There is probably some evidence to the effect that output rate tends to be slightly depressed during the menstrual periods; but this is certainly not a major effect, and it is too small to be of account in considering fluctuations of working rate.[9]

To this can be added the work of Poffenberger and Rounds concerning the effect of noise on output. Poffenberger and Rounds, confirming the work of Laird, state:

When the noises were first introduced there was a temporary reduction in output of work and in accuracy. . . .[10]

It was found that both the reduction in output and the increase in energy cost were only temporary and that in a surprisingly short time the worker returned to a normal output and a normal cost per unit of work.[11]

Thus, a superficial interpretation of these results would lead to the conclusion that all of these influences of the mechanical environment have little, if any, effect on our state of statistical control. The chance cause system we have postulated as the foundation upon which rate setting rests, remains undisturbed. It remains constant, and the job times of individual workers remain completely predictable from the sample frequency distributions.

However, this interpretation falls short in that it fails to in-

[6] *Ibid.*, I, 73.
[7] *Ibid.*, I, 85.
[8] *Ibid.*, I, 50.
[9] *Ibid.*, I, 53.
[10] A. T. Poffenberger, *Principles of Applied Psychology* (New York: D. Appleton-Century Co., 1942), p. 134.
[11] *Ibid.*, p. 136.

quire what is going on within the man-machine system itself. It is judging a condition by a purely outward manifestation. It is very much like assuming that a man who seems able to walk normally after a railroad wreck is actually perfectly normal. X rays may show some obscure internal injury that will not become apparent until much later.

In developing a time study system, it is wise to recall that the founder of the modern usage of the technique, Frederick W. Taylor, always emphasized that the task for an individual worker must be set so as not to impair his health. Taylor made that much plain in his congressional testimony when he stated:

> I think the interest of every man who is in any way engaged in scientific management . . . must be first the welfare of the workingman.[12]

> In time and action study it [scientific management] has discovered and developed an accurate scientific method by which the great masses of laws governing the most productive movements of men are registered. . . . Scientific management guards the workers against overspeeding and exhaustion, nervously and physically. . . .[13]

Thus, all of the conclusions of Poffenberger and Whitehead imply evidence for time study techniques only if it can be demonstrated that this maintenance of output or increase in output, as the case might be, actually reflects a satisfactory fatigue rate, or, to use a better term, physiological tax, in view of the disrepute into which the term fatigue has fallen.

Poffenberger attempts to show that the physiological tax remained the same because the metabolic rate of the subjects returned to normal.[14] Whether or not metabolic rate is an adequate measure of the physiological tax will be examined in the next chapter.

There are three ways of interpreting what is happening within the man-machine system. One interpretation would be that no

[12] *Final Report and Testimony Submitted to Congress by the Commission on Industrial Relations Created by the Act of Aug. 23, 1912,* I (Washington, D. C.; Government Printing Office, 1916), p. 766.

[13] Robert Franklin Hoxie, *Scientific Management and Labor* (New York: D. Appleton & Co., 1915), p. 144.

[14] Poffenberger, *op. cit.*, p. 136.

physiological tax is being imposed upon the worker and that, as a consequence, we may conclude that all of these facts of the material environment are immaterial to the laborer. On the other hand, it may be that other forces have come into play which overwhelm the effects of physiological tax to such an extent that this burden, which is very real, just does not show itself in output changes. There is still a third possibility—that the physiological tax itself is dissipated by other forces at work.

In addition to the effect of the four factors of the material environment mentioned on page 85, Whitehead called attention to the fact that the rate of increase of production was independent of whether or not rest periods were granted the workers during this experimental period.[15] Roethlisberger was anxious to test whether or not the fatigue hypothesis accounted for the increased output.[16] He observes that "even though the major output change could not be attributed to this or that type of working day, nevertheless the rest pauses . . . had provided a relief from cumulative fatigue."[17] Recognizing the possible validity of this argument, Roethlisberger describes a method of testing the fatigue hypothesis. The analysis of the validity of his conclusions must await an analysis of the nature of fatigue in industrial situations, the subject matter of our next chapter.

In summarizing the effects of the mechanical forces of the environment upon the constant chance cause system governing the variation of the production standard, we find that mechanical forces can be divided into two classes:

1. Those that interfere with the regularity of the repetitive operation and present no problem except a statistical analysis to account for their occurrence;

2. Those whose effects are more subtle. They are suspected of affecting the fatigue accumulation rate of the worker and require a physiological analysis before any conclusion can be drawn about their effect upon the constant chance cause system.

[15] Whitehead, *op. cit.*, I, 42.
[16] Roethlisberger and Dickson, *op. cit.*, pp. 87–88.
[17] *Ibid.*, p. 87.

CHAPTER 7

Accounting for the Fatigue Factor

As has been previously mentioned, one of the principal factors affecting the operation of the constant chance cause system in time study is the influence of fatigue on the rate of work.

The problem of industrial fatigue affects every field of industrial operations. If the job is machine-paced the problem becomes one of setting the machine speed to yield maximum productivity with a minimum fatigue accumulation or at least a rate of fatigue that will have no deleterious effects on the health of the workers.

If the job is man-paced the engineer must know what working pace can be expected from a group of workers that will not dangerously increase their rate of fatigue accumulation.

However, attempts to reduce this common-sense concept to a scientifically objective, measurable phenomenon have not met with signal success until very recently. Detailed problems that time study engineers faced were: How great shall an allowance for fatigue be? How shall the size of the allowance be correlated with the time of day during which the basic time observation has been observed? Shall the fatigue allowance be an allowance for an actual rest period or shall we merely add it to the standard on the assumption that it is a compensating device for a necessary change in pace? Must a solution wait until the physiologist and pyschologist can inform the engineer just

what objective criteria of the phenomenon are available and how they can be measured? It is only when this basic information is forthcoming that engineers can begin to talk about an objective scientific allowance for fatigue in their rate-setting techniques.

The confusion and unwarranted claims surrounding the treatment of the fatigue factor in present time study practice are well illustrated by a case before the War Labor Board to which one of the locals of the International Ladies' Garment Workers' Union was a party during 1944.

A belt factory in contract with the International Ladies' Garment Workers' Union found itself unable to attract any new workers to its pay roll and unable to hold many of its old workers. Restrained from raising wages by the war stabilization measures in force, the management decided to install a wage incentive payment plan which would offer workers increased earning opportunities in return for higher production. In the course of securing approval for the completed plan after it had been designed and installed by a nationally known consulting engineering agency, a meeting was arranged by the War Labor Board consultant, including union representatives and the senior engineer in charge of installation. The representative of the Board, in reviewing the methodology behind the production standards underlying the plan, questioned the fatigue allowance of 18 2/3 per cent which had been added to the basic time study, asking whether this fatigue allowance was a slowdown allowance or an allowance for actual rest periods.

The senior engineer, without attempting to ascertain exactly what information was requested, declared that 18 2/3 per cent was the result of the engineering organization's "scientific" investigation of the job's fatigue requirement. The War Labor Board's representative was kind enough to refrain from asking the engineer for the basic data supporting his figure. This sequence of events is altogether too typical.

Even a perfunctory investigation of the field of fatigue study discloses that recognized medical authorities and physiological

investigators as late as 1940 were stating that "fatigue is still a source of bewilderment to the scientist—it cannot be defined in simple terms and nobody knows how to measure it."[1]

This problem of the relationship of industrial fatigue to time study can best be understood from a review of the historical context within which both concepts were developed.

Taylor, in his classic description of the time study technique that established the foundations of modern time study methods, made a combination factor of fatigue and downtime one of the principal variables in measuring a correct production rate. It will be recalled that this description depicted the measurement of the amount of work that a first-class barrow shoveler should be able to perform during the time of a normal work day.[2] Taylor specified every detail of the man's job: the size of the shovel; the bite of the shovel, that is, the weight of the scoop; the distance that the man was to walk with the loaded shovel; the distance with the empty shovel; the swing of the throw; and, finally, a specified rest period at regular intervals that the man was compelled to take. In computing the fatigue allowance for the job, the amount of time absorbed by these rest periods, plus downtime, divided by the total amount of time, multiplied by 100, constituted the fatigue downtime requirement for the job.

It will be noted that fatigue was not separated as an allowance by itself, but combined with downtime for nonrepetitive elements to make an over-all combined allowance to which Taylor assigned the letter *P* in his fundamental formula for shoveling sand. This was really a combined physiological-mechanical allowance. By varying every one of these factors, he established that combination of factors which yielded the maximum amount of material consistently loaded on the barrow by the shoveler. This was very similar to the method that Taylor

[1] C. O. Sappington, "Developing Fatigue Factors for Time Study Work," [1940] *Proceedings of the National Time and Motion Study Clinic* (Chicago: Industrial Management Society, 1941), p. 10.

[2] Frederick Winslow Taylor, *Shop Management* (New York: Harper & Bros., 1911), p. 159.

used earlier to establish the optimum conditions of feed and speed of machines for the cutting of metals. In this latter field he had been extremely successful, and the publication of his paper, *The Art of Cutting Metals,* had established his reputation as the foremost thinker in this field and laid the foundation of modern machine-shop practice. The artificial transference of this method from the study of machine tools to the study of the human being presupposed that an adequate measure of worker fatigue would be comparative productivity, provided the human being himself showed no superficially visible symptoms of undue physical strain.

This implicit assumption that an adequate measure of fatigue could be found in the measure of output has persisted in industrial engineering thinking right down to the present day. It has even colored a good deal of the thinking of the members of the field of industrial psychology, the foundations of which were being laid concurrently with those of scientific management by Hugo Münsterberg[3] at Harvard and J. McKeen Catell at Columbia.

Naturally, since Taylor was primarily interested in maximum output, he was interested in selecting those individuals best adapted to a job, those who would yield maximum work and show minimum fatigue. Hence, on the basis of a careful selection of his workers—that is, a carefully stratified sample, to revert to statistical terminology—Taylor was able to set a minimum fatigue allowance. However, the social cost of this kind of efficiency is best understood by a recapitulation of the outstanding characteristics of the qualifications of the workmen making up this stratified sample. Taylor stated, "Now one of the very first requirements for a man who is fit to handle pig iron as a regular occupation is that he shall be so stupid and so phlegmatic that he more nearly resembles in his mental make-up the ox than any other type."[4] Naturally, if we guide the ne-

[3] Hugo Münsterberg, *Psychology and Industrial Efficiency* (Boston: Houghton Mifflin Co., 1913).

[4] Frederick Winslow Taylor, *Principles of Scientific Management* (New York: Harper & Bros., 1911), p. 59.

cessities of a fatigue allowance by this sort of selection, the simplification of jobs at one end of the job-rating scale and the increasing demands for skills at the other end would soon create a society in which only the geniuses and morons would survive. It would make as pretty a picture as Aldous Huxley paints in his *Brave New World.* In setting a fatigue allowance for any task today, we must proceed on the assumption that the people whom we find in any task constitute the sample of the statistical population from which we must draw our inferences, unless we can find more adequate tasks for these people elsewhere. This is an unlikely assumption in the labor surplus economy in which we usually operate and is a concept that will be examined in our Chapter 13 on rating techniques.

Practically all industrial engineers carry on this tradition and relate the fatigue concept to a reduced capacity for work. Holmes defines fatigue "as the overuse of the mind or muscles, resulting in a lack of energy and an inability to continue a set pace or speed."[5] Schutt does not find it necessary to examine the concept too deeply. He dismisses it with instruction to the reader that a human being is not an automatic machine and an allowance should be made for fatigue. Exactly what the allowance is, how it should be measured, and on which basis it should be made—these are left to the reader's imagination. Schutt does suggest that the new engineer should find out what past practice has been in the factory on these allowances, but he raises no questions about how these allowances were determined in the first place.[6]

Shumard, without attempting to define fatigue, implies a definition. He recommends that the problem posited by time study can be solved in one of two ways, either by a direct method or an indirect method. He goes on to say:

> The direct way solves the problem by actually timing the fatigue that accrues from each job as it is encountered. The indirect way relates to the

[5] Walter G. Holmes, *Applied Time and Motion Study* (New York: The Ronald Press Co., 1938), p. 120.

[6] William H. Schutt, *Time Study Engineering* (New York: McGraw-Hill Book Co., 1943), p. 60.

use of general reference fatigue data prepared after extensive research on various kinds of jobs. This latter way may in many cases prove to be such a prolonged process that it may not justify the expense involved.[7]

Finally becoming very practical, Shumard lists a number of rest percentages to be used on various kinds of work without giving any supporting data to justify his conclusions. Here again the implication is that fatigue is measurable in terms of changes in daily output curves.

Presgrave discusses both directly and indirectly the use of the fatigue concept in his critical evaluation of the time study field. His indirect reference to the concept is made in his evaluation of the Barth-Merrick correction factor as an allowance for combined downtime and fatigue in building production standards from time studies.

Dwight V. Merrick, together with Carl Barth, one of Taylor's earliest and closest collaborators, worked out a series of fatigue downtime allowance curves, since abandoned by time study practitioners. These curves were drawn on the assumption that the ratio of actual time that a worker spent setting up a machine to the machine running time during which the worker was presumably idling or watching the machine was the critical factor in determining what the fatigue allowance should be. These curves were drawn from experience in machine shops, and they therefore show the predominating influence of machine-shop practice. The implicit assumption of the division of the work cycle into set-up time, running time, and put-away time is apparent.

The shapes of the curves which are illustrated in Figure 4 indicate that these curves were constructed on the evident assumption that the worker was resting during the machine running elemental time. Thus the supposition that the over-all allowance should vary inversely with the percentage of machine running time is not entirely as illogical as Presgrave indicates.[8]

[7] F. W. Shumard, *A Primer of Time Study* (New York: McGraw-Hill Book Co., 1940), p. 242.

[8] Ralph Presgrave, *The Dynamics of Time Study* (2d ed., New York: McGraw-Hill Book Co., 1945), p. 14.

L. C. Kerkhoven of Holland is the only modern exponent of Barth's basic approach. He founds his system upon what he calls his scientific fatigue studies. His approach is based upon Kennelly's law for the relationship between the speed of racing animals and the length of time during which the speed can be sustained.

$$Vt = \frac{K}{T^{\frac{1}{8} - \frac{1}{9}}}$$

Vt = speed or pace
K = constant
T = elapsed time

He claims that the Industrial equivalent of Kennelly's Law is an equation that he has derived for industrial work. It is

$$tx = t\left(\frac{t}{T}\right)^{n}$$

tx = standard time for the job
T = hours per day of work
t = single cycle a day time
n = exponent which varies with each type of worker and kind of work.

The "n" is said to vary from 0 to −0.2. The single cycle a day time is the performance time of one single operation at top speed.

Kerkhoven attempts to show that his formula and the Barth curves give the same results for the 10 per cent handling time and 90 per cent machine time curves and for the 0 per cent handling and 100 per cent machine time curves. Kerkhoven does not give any satisfactiory method according to Cohen,[10] however, for the derivation of t or n, the so-called record time for the job, or n, the so-called fatigue allowance. This leaves us right where we started. It is also an interesting example of the direct application of management's philosophy of a fair labor

[9] L. C. Kerkhoven, "Time Study Research," *Advanced Management,* Vol. XVII, No. 11, November 1952, p. 23.

[10] Abraham Cohen, *Time Study and Common Sense* (London: MacDonald & Evans, 1947), p. 17.

standard. Kerkhoven expects a level of output utilizing the full physiological limit of a worker's capacity.

The blind application of these formulae inspired by Barth, without any thought to their rational base, has led astray many practitioners of modern time study. The *Cost and Production Handbook* recommends that the extension of these curves, with some modifications in the basic algebraic formula, to such operations as cutting. This seems to lack any rational base. Garment cutting, to take only one instance, requires more intense attention during the time that the machine is operating than during the laying-up of the cloth, the equivalent for the set-up time in the machine shop.

As the time study technique was extended to other industries, a subtle but unexpressed change was taking place in the concept of the fatigue allowance. The percentage added to the observed time was no longer understood as an actual rest period but was a compensation for an implied slowdown loosely covered by the term fatigue. This change in concept has persisted down to the present day. It is a subtle recognition that industrial operations have changed in pattern and no longer require much of the muscular expenditure characterizing jobs in Taylor's day. However, as Viteles points out, the increased tension of work more than offsets the muscular exertion required previously.[11]

Just how the fatigue allowance should be divided between an allotment for change in pace and a share for actual rest periods remains an open question. Vernon has demonstrated that rest periods markedly improve output even for jobs requiring a minimum of physical exertion.[12] They seem particularly effective on repetitive jobs where they afford a relief from boredom. But here again there is an implied assumption that comparative output is an adequate index of fatigue.

The direct reference made by Presgrave to the allowance for

[11] Morris S. Viteles, *Industrial Psychology* (New York: W. W. Norton & Co., 1932), p. 439.

[12] H. M. Vernon, *The Health and Efficiency of Munition Workers* (London: Oxford University Press, 1940), pp. 36–37.

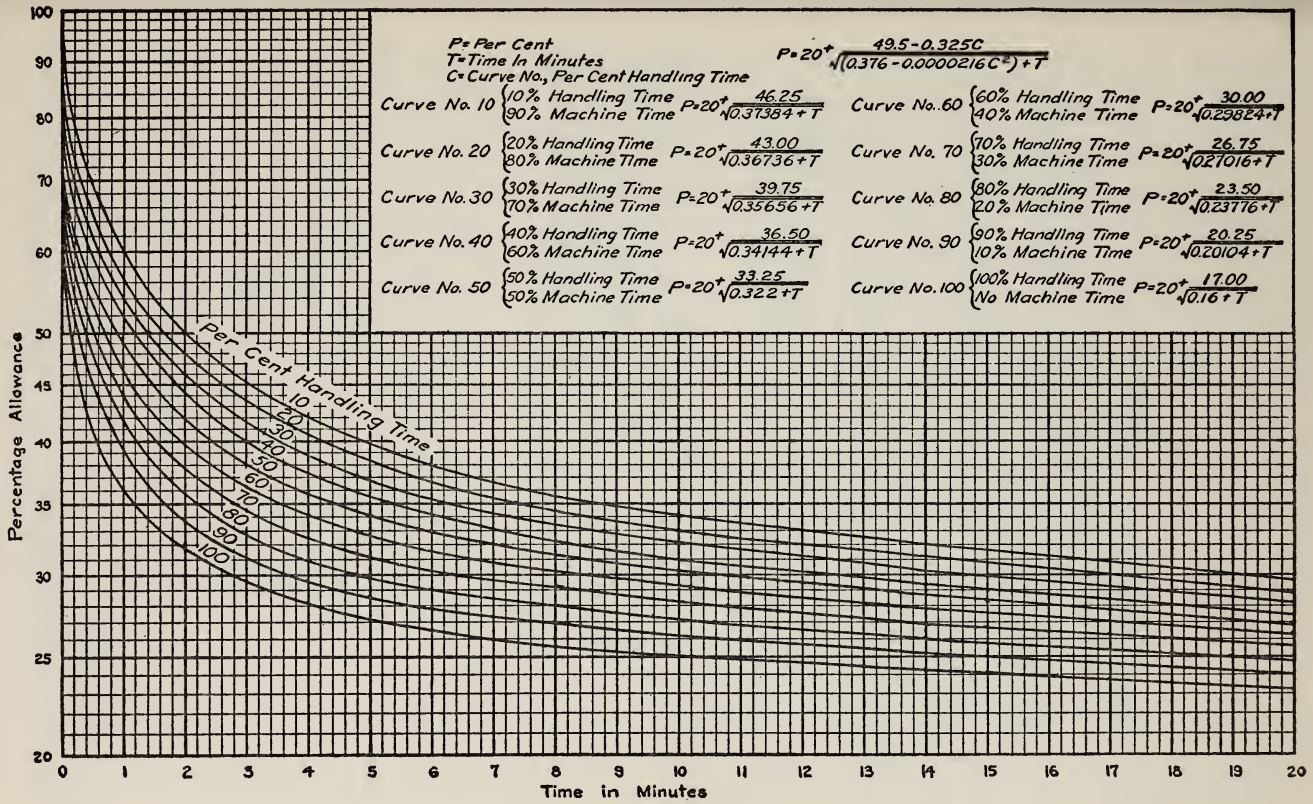

P = Per Cent
T = Time in Minutes
C = Curve No., Per Cent Handling Time
$P = 20 + \frac{49.5 - 0.325C}{\sqrt{(0.376 - 0.0000216C^2) + T}}$
Curve No. 10 {10% Handling Time, 90% Machine Time} $P = 20 + \frac{46.25}{\sqrt{0.37384 + T}}$
Curve No. 20 {20% Handling Time, 80% Machine Time} $P = 20 + \frac{43.00}{\sqrt{0.36736 + T}}$
Curve No. 30 {30% Handling Time, 70% Machine Time} $P = 20 + \frac{39.75}{\sqrt{0.35656 + T}}$
Curve No. 40 {40% Handling Time, 60% Machine Time} $P = 20 + \frac{36.50}{\sqrt{0.34144 + T}}$
Curve No. 50 {50% Handling Time, 50% Machine Time} $P = 20 + \frac{33.25}{\sqrt{0.322 + T}}$
Curve No. 60 {60% Handling Time, 40% Machine Time} $P = 20 + \frac{30.00}{\sqrt{0.29824 + T}}$
Curve No. 70 {70% Handling Time, 30% Machine Time} $P = 20 + \frac{26.75}{\sqrt{0.21016 + T}}$
Curve No. 80 {80% Handling Time, 20% Machine Time} $P = 20 + \frac{23.50}{\sqrt{0.23716 + T}}$
Curve No. 90 {90% Handling Time, 10% Machine Time} $P = 20 + \frac{20.25}{\sqrt{0.20104 + T}}$
Curve No. 100 {100% Handling Time, No Machine Time} $P = 20 + \frac{17.00}{\sqrt{0.16 + T}}$
Per Cent Handling Time
10
20
30
40
50
60
70
80
90
100
Percentage Allowance
Time in Minutes

FIGURE 4*

* Dwight V. Merrick, *Time Studies as a Basis for Rate Setting* (New York: Engineering Magazine Co., 1919), p. 17, reproduced by permission of the publisher.

fatigue in the production standard recognizes the necessity for a distribution of this allowance among intermittent rest periods taken by workers in the course of their work, regular organized rest periods, and provision for the change in pace during the working day.[13] However, he expresses discouragement with the failure to isolate fatigue as a separately measurable phenomenon. His final recommendation that the time study man be relieved of any responsibility for fatigue allowance in his capacity as a rate setter hardly constitutes a solution to this problem. His injunction that the whole problem be tackled by means of organized recesses incorporated into the basic wage payment plan and thus removed to the province of the wage administrator is an attempt to solve a problem by transference. It reminds one of the biologists who thought they had solved the problem of the origin of life on the earth by speculating that it might have come from another planet and been transplanted here by a meteor from interstellar space.

Ultimately, the wage administrator must consult the rate setter on what, in his opinion, the length of the rest period ought to be. If he does not have to solve the problem in his function as a rate setter, he will be called upon to solve the problem in his function as an advisor. In either case, his ignorance of the field will seriously limit his rate-setting function.

Ralph M. Barnes' references to the use of the fatigue allowances for time study discount its importance. After a carefully written chapter in the methods study section of his book on the means for eliminating fatigue, he evidently feels that if these directions are conscientiously followed, the fatigue allowance can be completely eliminated from time study practice in the modern factory except on those jobs requiring excessive physical exertion. His reasoning is based upon a quotation from the work of Professor A. G. Anderson of the University of Illinois who had spent a year studying the effects of fatigue upon workers in a midwestern factory.

Anderson stated:

[13] Presgrave, *op. cit.*, pp. 222–223.

> The general and final conclusion of this study of human fatigue is, then, to the effect that industrial operations as carried on in a modern, progressively managed manufacturing plant do not subject the workers to undue fatigue, either physical or mental, and that fatigue is not a factor tending to limit production.[14]

Barnes then goes on to state, "There is at the present time no satisfactory way of measuring fatigue except in terms of reduced output resulting from work.[15]

Lowry, Maynard, and Stegemerten merely mention the necessity of making an allowance for fatigue in terms of necessary rest periods for physically heavy work.[16] No attempt is made to analyze the nature of the fatigue. Its assignment as an allowance is left largely to the judgment of the time study observer. Here again by implication the sole measure of fatigue remains comparative output.

Lichtner follows the same reasoning as Barth and Merrick. The fatigue allowance is made a function of the relationship between handling time and machine time (see Fig. 4, p. 108). Inasmuch as we are confining our attention to man-paced operations, the allowances granted by Lichtner would be in the higher brackets.[17] Lichtner does not define exactly what is being measured and leaves the reader without any clear idea except that a competent analyst should be able to account for fatigue.

Throughout the writings of the time study engineers on the nature of fatigue, then, is the explicit or implicit assumption that the gauge of fatigue is the measure of output over the period being studied. Finally, this concept of fatigue has received institutional acceptance from the industrial engineering profession by its incorporation into the authoritative *Production Handbook*. The 1944 edition of the *Production Handbook* de-

[14] Ralph M. Barnes, *Motion and Time Study* (2 ed., New York: John Wiley & Sons, 1940), p. 280.

[15] *Ibid.*, p. 280.

[16] Stewart M. Lowry, Harold B. Maynard, and G. J. Stegemerten, *Time and Motion Study* (3 ed., New York: McGraw-Hill Book Co., 1940), p. 253.

[17] William O. Lichtner, *Time Study and Job Analysis* (New York: The Ronald Press Co., 1921), pp. 109–117.

fines fatigue as "that effect of work upon an individual's mind and body which tends to lower his rate or grade of quality of production, or both, from his optimum performance."[18]

The industrial psychologists have taken over this definition of fatigue from the engineers and have performed a great many experiments to determine the fatigue output curve of a wide variety of jobs.

Some psychologists and physiologists tried to probe a little more deeply. Attempts were further made to correlate declines in productivity with some physiological and psychological function that could be measured objectively. Personal inquiries at the Harvard Fatigue Laboratory, made as late as 1944, elicited the information that no physiological changes of any significance could be detected in factory workers employed on average tasks during a working day. They excepted especially difficult physical tasks where metabolic changes could be detected similar to those observed on a subject pedalling a bicycle type of laboratory apparatus. This severe strain characterizes comparatively few industrial tasks today. No physiological changes could be detected as a result of mental fatigue.

This information is particularly significant when we consider the interpretation of the fatigue hypothesis first examined by Roethlisberger of Harvard as mentioned in the preceding chapter of this book. After examining the Relay Assembly Test Room findings, Roethlisberger concluded:

> There is no evidence in support of the hypothesis that the increased output rate of all these operators during the first thirteen experimental periods was due to relief from fatigue. This hypothesis, which at first seemed most plausible, had to be abandoned.[19]

This conclusion was based upon the assumption that relative output was a measure of fatigue. While it is true that there was some attempt to check these measures against blood pressure tests and vascular skin reactions for some objective measure of

[18] L. P. Alford and John R. Bangs, *Production Handbook* (New York: The Ronald Press Co., 1944), p. 493.

[19] F. J. Roethlisberger and William J. Dickson, *Management and the Worker* (Cambridge: Harvard University Press, 1939), p. 127.

fatigue, the crudity of these devices is reflected in the lack of consideration extended to them from the Harvard Fatigue Laboratory.

It may very well have been that the initial improvement in productivity did represent a relief from fatigue but that the subsequent failure of production to reach its old low levels when rest pauses were removed was caused by some more dominant force than fatigue itself or by some fatigue-compensating medium. These concepts will be examined in the chapters on the psychological and sociological sources of variation. Meanwhile, until a more objective measure of physiological tax is developed, we are justified in drawing no conclusions about the effects of fatigue in the Western Electric experiment. To measure fatigue in terms of output and conclude that the output changes are not caused by fatigue is confusing.

It becomes increasingly obvious that this restriction of the index of industrial fatigue to the rate of output was highly artificial. After rather extensive experimental work, investigators of the British Industrial Health Board, successor to the Industrial Fatigue Research Board, eliminated "Fatigue" from their title in 1930 because of the confusion that had grown up around the concept. They thereby gave final acceptance to the conclusions of Musico, who had urged as early as 1921 that the concept of fatigue be entirely eliminated from the scientific discussion of industrial work.[20] The shortcomings of comparative output as an index of the fatigue stress on the human organism have been apparent to many investigators. Poffenberger of Columbia demonstrated that the accomplishment of onerous tasks could be maintained at a high level—despite the deterioration of the worker's subjective feelings in the matter—provided there was an attractive enough incentive of the environmental factors. The results are illustrated in Figure 5. The indices of the feelings made themselves apparent by the use of profanity and threats to leave the job.[21]

[20] Viteles, *op. cit.*, p. 463.

[21] A. T. Poffenberger, *Principles of Applied Psychology* (New York: D. Appleton-Century Co., 1942), pp. 134–135.

Viteles tends to dismiss these subjective feelings on the grounds that they are poor indicators of fatigue.[22] Yet later studies, particularly the Hawthorne experiment, led one of its investigators, G. A. Pennock, to conclude that "mental attitude of operator toward supervisor is probably the biggest single factor governing the employee's efficiency."[23] Thus the subjective feelings in this case were a primary index of fatigue if the index of fatigue is keyed to comparative productivity.

The utter confusion to which the output index of fatigue leads is brought out by contrasting the work done and conclusions reached by the physiologists on the one hand, and the industrial psychologists on the other.[24]

Lehmann, in his studies on the finger ergograph, indicated that the normal type of individual shows a regular load curve decrease with the incidence of fatigue. His emphasis is on the gradualness of the process.[25]

Mayo, on the other hand, points out that current business theory had erected its wage structure on the assumption that work is something taken out of the individual in gradual increments; and that wages are paid to him for this vaguely conceived loss. This, of course, is the root idea of the industrial fatigue concept. However, Mayo argues that "the disaster is not slowly continuous as the wage theory implies; once it has appeared, the individual is rapidly forced to stop work."[26] Of course, implicit in this conclusion is an acceptance of the performance or rate of output as the fatigue index.

As we have seen, Mayo's conclusion naturally follows from the treatment of the fatigue concept in the Hawthorne experiment with which he was so intimately associated.

[22] Viteles, *op. cit.*, p. 457.

[23] Alford and Bangs, *op. cit.*, p. 494.

[24] Viteles, *op. cit.*, p. 444.

[25] The finger ergograph consists of a small key, the movement of which is restricted by a spring. Thus each depression of a finger results in the expenditure of a fixed unit of foot pounds of work.

[26] Elton Mayo, *The Human Problems of an Industrial Civilization* (Boston: Division of Research, Graduate School of Business Administration, Harvard University, 1946), p. 23.

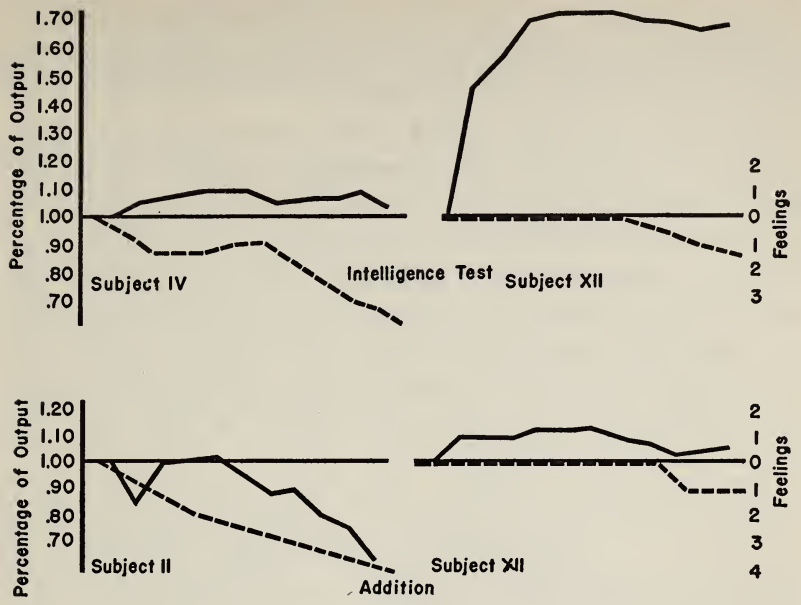


FIGURE 5*

* A. T. Poffenberger, *Principles of Applied Psychology* (New York: D. Appleton-Century Co., 1944), p. 112. Reproduced by permission of the author.

Tiffin attempts to solve this problem by redefining the fatigue concept into three separate categories: physiological fatigue, psychological fatigue, and industrial fatigue.[27]

Industrial fatigue is defined as whatever changes occur in a person that result in decreased employee production. The reader is expressly warned that this concept is independent of, and unrelated to, the other fatigue concepts which have their own measurement criteria.

Tiffin goes on to state:

> Fatigue is important in industry not to the extent that it involves physiological changes in the muscles, or feelings of boredom on the part of the employees, but, rather, to the extent that it involves on a long term or a short term basis a reduction in the employee's efficiency on the job. . . . The industrialist, therefore, *defines fatigue as whatever changes*

[27] Joseph Tiffin, *Industrial Psychology* (3d ed.; New York: Prentice-Hall, Inc., 1952).

occur as a result of work that result in decreased employee production.[28]

But, just as the physiological and psychological definitions of fatigue do not always agree with each other, so also the industrial definition does not always agree with either of these two.[29]

Now there can be no argument with the logical formulation of such a concept. However, any attempt to relate such an artificial logical formulation to the live industrial picture is of extremely limited usefulness. The pioneers of the scientific management movement always protested, as we have shown, that the rate of speed which they recommended was not so excessive as to injure the health of the worker. The artificial exclusion of physiological and psychological considerations from the area of industrial fatigue opens the whole method of measuring production standards to suspicion.

Tiffin himself is evidently aware of the extremely limited usefulness of his own definition of industrial fatigue when he describes the experimental results of G. F. Arps, paralleling the findings of Poffenberger. Arps found that a production curve can be maintained at a fairly high and constant level if incentives are sufficiently strong, even though physiological measurements indicate an unwise or extreme expenditure of energy.

It was this type of confusion that led Musico to recommend the abandonment of the fatigue concept as early as 1921.[30] Pointing to the wide number of interfering factors, such as motivation, he frankly doubted that any such objective measure as a fatigue test was possible. As late as 1944, Professor Ryan of Cornell pointed out that when we attempt to overcome the irrational linking of output to fatigue by defining fatigue as a reduced capacity to work, we are left without an adequate experimental procedure.[31] He expressed the fear that our common usage may have led us to assume the existence of an entity where none exists.

[28] *Ibid.*, p. 219.
[29] *Ibid.*, p. 220.
[30] B. Musico, "Is a Fatigue Test Possible?" *British Journal of Psychology*, XII, Part I, June 1921, 31–46.
[31] T. A. Ryan, "Varieties of Fatigue," *American Journal of Psychology*, LVII, October 1944, 565–69.

Ryan is completely discouraged with any attempt to make a fatigue allowance part of a standard setting procedure. Instead he suggests the use of his concept of effort. However, an examination of his concept of effort and its application to standards setting seems to leave as many problems unsolved as equivalent attempts to measure fatigue. He defines effort subjectively as follows: "By effort we shall mean an individual's experience of how hard he is working." He recognizes that some more objective physiological test is necessary as an indication of fatigue. He suggests that the future answer may lie in researches on the correlations between the level of muscular tension and the electrical discharges in the muscle tissue. He also suggests possible correlations between suggested levels of activity and the electrical resistance of the skin. However these studies, it is acknowledged, are in their pioneering stages and as yet offer little useful information for actual rate setting.[32]

Bitterman suggests a new measure of fatigue that would overcome the interfering influences of motivation and other extraneous factors. He proposes the invention of a new measure, the ratio of performance to expended effort, as an index of fatigue. However, he does not make clear just what his objective measure of effort would be and how it would be subjected to an experimental procedure.[33]

Bartley and Chute suggest that fatigue may be defined as "An experimental pattern arising in a conflict situation in which the general alignment of the individual may be described as aversion." Impairment on the other hand is used where specific tissue conditions arise. Thus fatigue is defined as an over-all term including the emotions, and impairment as a purely physiological term.[34] The authors decry any attempt to make output an index of either fatigue or impairment. They specifically state

[32] Thomas A. Ryan, "Fatigue and Effort," *International Labor Review*, Vol. 45, No. 1, January 1952, pp. 44–51.

[33] M. E. Bitterman, "Fatigue Defined as Reduced Efficiency," *American Journal of Psychology*, Vol. LVII, October 1944, pp. 469–73.

[34] S. Howard Bartley and Eloise Chute, *Fatigue and Impairment in Man* (New York: McGraw-Hill Book Co., Inc., 1947), pp. 47–48.

that there is no way of measuring fatigue in quantitative units.

The conflict situation which Bartley and Chute have in mind is an emotional complex at the basis of much modern psychoanalysis and psychiatry. The work situation is only one of the contributors to this complex and may very well be a minimum contribution. This, however, does not relieve the employer of the responsibility of considering this factor when he demands a specified level of output. If he requires human beings for his work task, he has to take them as they come in a human way with all their human problems.

A completely new approach to the fatigue problem seems to have been developed by two American physiologists, Drs. Hudson Hoagland and Gregory Pincus of the Worcester Experimental Biological Station, Shrewsbury, Massachusetts. The distinctive approach made by Hoagland and Pincus in attempting to solve the problem of fatigue can best be understood by a study of the basic observations with which Hoagland begins the development of their theory. He says:

> Man together with other vertebrates has developed some elegant automatic mechanisms for regulating the physical and chemical properties of his blood and body fluids. *The relative consistency of one's internal environment* in the face of external stress and change is characteristic of such factors as blood volume, blood sugar, hydrogen, iron, concentration and salt content of the body fluids.[35]

This approach is distinguished from those of all predecessors who have tackled the fatigue idea. This difference can perhaps best be understood by an analogy with a steam engine.

Let us assume that a mechanical engineer wished to study the stress characteristics of a steam engine whose speed is controlled by a centrifugal governor. Forgetting all about the governor, he imposes a load of a fixed number of foot-pounds upon the engine, records the machine speed, then, in successive fixed increments, increases the load upon the machine, watching machine speed against each successive load increment. Finally, he im-

[35] Gregory Pincus and Hudson Hoagland, "Adventures in Biological Engineering," *Science,* July 28, 1944, p. 63.

poses a load upon the engine so far beyond its designed capacity that the engine breaks down. He then concludes that the wear and tear upon the engine is independent of the load imposed until the time when the load becomes so high that the engine fails to function. The fallacy of the argument is at once apparent to any trained engineer, or, for that matter, to any alert mechanic. Despite this, is not a similar fallacy basic to the reasoning of most students of fatigue?

The competent engineer in the study of the engine cited above would at once reply that we have taken a fallacious indicator of machine wear and tear. In all likelihood, he would recommend a metallurgic study of machine parts subjected to wear. How well do these various parts maintain their dimensions after a sustained period of carrying a fixed load? How well does the metal resist crystallization or softening under the temperature to which it is subjected? Quite obviously the speed is no indicator of wear and tear on the mechanism. It is held constant by the governor until the machine load is beyond its rated capacity.

Now while it is possible to disassemble the parts of a machine and put them together again so that the machine will suffer no ill effects, obvious biological limitations prevent us from making a similar study of human beings. Thus students of fatigue have been attempting to analyze the phenomenon with somewhat disappointing results. The physiologists have been measuring functional indicators, most of which have remained constant under body stress until a sudden pathological breakdown takes place. Thus Mayo's misinterpretation of the fatigue phenomenon becomes understandable.

It remained for Hoagland and Pincus to look for the solution of these fatigue problems in the governing apparatus of the human mechanism.[36] After extensive physiological investigation, both Hoagland and Pincus came to the conclusion that the measure of physiological stress lay in the excretion of the break-

[36] The interpretation of Professor Hoagland's findings which follows has been checked in an oral interview with him.

down products of the endocrines secreted by the adrenal cortex. These compounds were known as 17-ketosteroids.

It will be recalled that Mayo made much of the fact that no measurable degree of fatigue had been developed biologically until the pathological breakdown point in the human being appeared. But here we see at once that the fatigue buildup may have been cumulative but invisible. All our biological methods have thus far failed to penetrate the governing mechanism of the human body. Though some of Hoagland's and Pincus' work attempted to prove results by correlating output with ketosteroid excretion, one particular experiment with two sets of aviators seems to indicate a more objective measure of fatigue than has hitherto been available.[37]

The ketosteroid excretion of two sets of aviators was measured, one an Army group and one a group of Pratt and Whitney test pilots. The percentage of time that each of the groups was in the air was measured. This is presumed to be a measure of stress on the individual. It is very similar to industrial tasks in that it does not call for great physical exertion but does call for some mental stress. The results that the two investigators found are graphed in Figures 6 and 7.

Figure 6 correlates the percentage excretion of ketosteroid of the regular group of pilots with percentage time in the air. The data represent readings on sixteen subjects for 152 flights. The correlation coefficient between percentage time in the air and ketosteroid output is 0.978 plus or minus .01.[38] Figure 7 portrays a comparison of the Army pilot group with the test pilot group. Note that the group variation is practically parallel but at a higher level. This very logically accounts for the added stress on the test pilots.

Similar ketosteroid studies of industrial workers were being undertaken at the time this book was being written. There is every reason to believe that this concept can be extended to

[37] Gregory Pincus and Hudson Hoagland, "Steroid Excretion and the Stress of Flying," *Journal of Aviation Medicine,* August 1943, pp. 173–93.

[38] Pincus and Hoagland, "Steroid Excretion . . .," *op. cit.,* p. 186.

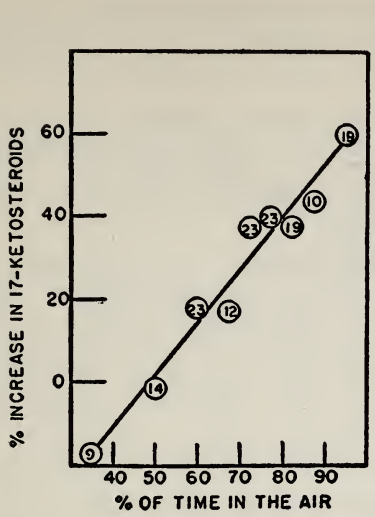


FIGURE 6*

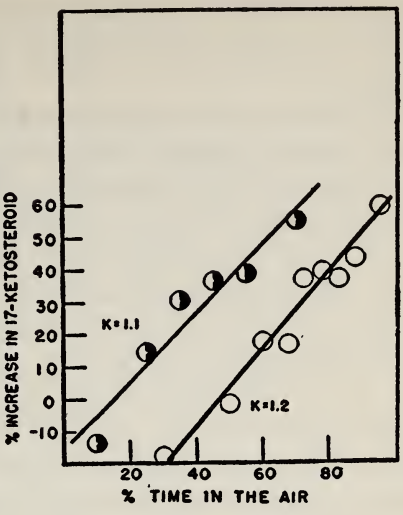


FIGURE 7*

* Gregory Pincus and Hudson Hoagland, "Steroid Excretion and the Stress of Flying," *Journal of Aviation Medicine,* August 1943, p. 186, reproduced by permission of the publisher.

industrial workers. A companion experiment that throws some light on the effectiveness of ketosteroid output as a measure of industrial fatigue has already been performed. In the course of their experimentation upon the aviators, both Hoagland and Pincus were thinking in terms of some remedy for the physiological tax imposed upon them by their work. A new compound, Delta 5 pregnenolone, was developed which, it was assumed, would compensate for the loss of the steroids from the body. This new compound was tried out on the group of aviators and its positive results are described in the April, 1944, issue of *The Journal of Aviation Medicine.*[39]

The manner in which this pregnenolone works may be interpreted in two ways. If the steroids are actually a bodily substance, a low supply of which shows itself in symptoms of fatigue in the same way that a food shortage of the body would

[39] Gregory Pincus and Hudson Hoagland, "Effects of Administered Pregnenolone on Fatiguing Psychometer Performance," *Journal of Aviation Medicine,* April 1944, pp. 98–115, 135.

make itself felt in hunger, then the replacement of the steroids by this pregnenolone supplies a bodily need that overcomes fatigue.

If, on the other hand, the steroids are interpreted as governor mechanisms charged with giving out warnings when the body is being overtaxed, then the replacement of the steroids by the pregnenolone acts as a mere stimulant.

Hoagland and Pincus have administered this pregnenolone to a group of industrial workers to determine its effects.[40] The study was made on a group of eight men and four women workers who were receiving piece work rates turning out bayonets on turret lathes. The entire group was under the impression that they were being given a new vitamin type of pill. Actually they were given an inactive placebo pill during one period and the pregnenolone pills during other periods. Both pills tasted the same so that motivation was eliminated as a possible course of variation in production. The tabulated results are given in Table II.

TABLE II*

THE MEAN DAILY BAYONET PRODUCTION IN THE TWO GROUPS OF WORKERS

Grp. No.	No. oper.	Medication	Month	Wk. No. 1	Wk. No. 2	Wk. No. 3	Wk. No. 4	Wk. No. 5
1	6	None	Feb.	121.0	124.5	128.5	134.7	....
2	6	None	Feb.	128.7	128.5	131.0	134.4	....
1	6	Placebo	Mar.	132.2	150.3	154.9	150.1	....
2	6	Pregnen.	Mar.	139.8	152.1	155.7	149.1	....
1	6	Pregnen.	Apr.	147.2	153.1	157.9	159.5	158.1
2	6	Placebo	Apr.	144.5	157.0	148.3	150.1	151.7
1	6	Placebo	May	163.8	143.2	131.4	137.7	....
2	4	Pregnen.	May	151.8	165.8	158.6	159.2	....
1	5	None	June	143.4	143.5	150.1	146.7	....
2	4	None	June	144.3	148.8	155.5	145.4	....

* Gregory Pincus and Hudson Hoagland, "Effects on Industrial Production of the Administration of Delta 5 Pregnenolone to Factory Workers, 1" *Psychosomatic Medicine,* Vol. VII, No. 6, November 1945, p. 343. Reproduced by permission of the publisher.

[40] Gregory Pincus and Hudson Hoagland, "Effects on Industrial Production of the Administration of Delta 5 Pregnenolone to Factory Workers, 1" *Psychosomatic Medicine,* Vol. VII, No. 6, November 1945, pp. 342–346.

It will be noted from the figures that the increases under the influence of pregnenolone exceed the increases when the placeboes are administered. The placeboes cause an increase in productivity beyond the level when no drug is administered. This may be interpreted as the result of a new social situation similar to that of the Relay Assembly Room of the Western Electric experiment. The difference, however, in production between the administration of pregnenolone and placeboes can be traced to the steroid replacement. This experiment is not so important to the purpose of this study in showing what the effects of pregnenolone are as it is in adding evidence to justify the use of ketosteroid output as a measure of industrial fatigue.

In a sense it can be argued that the interpretation of our results is limited by use of output as a criterion of fatigue. We are doing exactly what we said that engineers do who misunderstand the nature of circular reasoning. This would hold true if we were using this data for a quantitative measure of actual physiological expenditure. However, all that we are attempting to do is isolate a qualitative effect of the pregnenolone. Under these circumstances we can argue that no difference in output would not necessarily lead to the conclusion that the drug did not effect physiological expenditure. However, a difference in output under a wage incentive payment plan does indicate much more strongly that there is an effect on physiological expenditure.

It is for this reason that we are inclined to dismiss the significance of the experiments made by Graham-Bryce, A. K. Bullen, and W. H. Forbes on workers doing dull and exacting work with a very low motivation.[41] These investigators came to the conclusion that the ketosteroid output index of fatigue was insignificant industrially.

Some new experimental work in Korea once again emphasizes the ketosteroid output as a measure of physiological tax.

The significance of the ketosteroid measure as an index of

[41] Graham-Bryce, A. K. Bullen, and W. H. Forbes, "Effect of Pregnenolone on the Ability to Perform Prolonged Psychomotor Tests."

related physiological and psychological stress is pointed up by a later study made of soldiers under combat in Korea by a team of research scientists. The team was composed of physiologists, psychiatrists, and psychologists. The ketosteroids, it will be recalled, are the final breakdown hormone products thrown off by the pituitary-adrenal system. One of the many conclusions reached in this study revealed that: "The function of the adrenal gland, particularly the adrenal cortex, in frontline infantrymen not in active combat, is normal, while following severe combat stress, there is a prolonged, high level of adrenal activity."[42]

It still remains problematical whether or not the ketosteroid approach to the measurement of fatigue will be fruitful. Some way will have to be found which isolates the physiological and psychological tax imposed by work upon the workers from the tax imposed by the normal stresses and strains of the emotional problems he faces at home. It is anticipated that the Worcester Foundation for Experimental Biology will undertake such studies in the future. The adrenal cortex and its hormones are closely associated with various emotional states. The ketosteroid index of fatigue, if successful, would lead to the disappearance of the artificial barrier between the concepts of physiological and psychological fatigue.

Should these results be as positive in the industrial field as they are in the aviation field, then industrial engineers will be able to get actual objective measures of fatigue under industrial conditions. The only equipment that is necessary in the factory for carrying out a series of tests of this nature is a receptacle for gathering urine at fixed time intervals. Correspondence with the Messrs. Hoagland and Pincus during March, 1946, discloses that there is an even simpler technique of detecting these fatigue products through an ear lobe test, thus dispensing with the inconvenience of collecting urine receptacles. All that is

[42] "A Study of Stress, Korea 1952 (Preliminary Report)," Operations Research Office, Johns Hopkins University, operating under contract with the United States Army in co-operation with the Office of Naval Research.

necessary in this new test is that the ear lobes be pricked for a blood specimen at fixed time intervals.

In summarizing the effects of the physiological sources of variation on the constant chance cause system, we find that there is very little that we know. A great deal of work has been done that is tautological in character. This has been the result of attempting to measure the physiological work tax in terms of relative output. The new development by Hoagland and Pincus will require more industrial experimentation before we can gauge its possibilities of application. Meanwhile we must confess that we have no way of knowing exactly how the physiological effects of work affect the constant chance cause system upon which a scientific rate-setting structure must be built.

Once we have developed an objective measure of physiological tax, then a great many other problems will open up in terms of the long-run effects of work upon the man in the man-machine system. Effects of work upon the aging process and upon the general health of the worker are problems that may be explored and lead to the development of new criteria in setting a production standard.

The point at which the physiological sources of variation merge into the psychological sources of variation is not clear-cut but very shadowy. While reviewing the subject of fatigue, we have seen that some investigators have even attempted to define the concept of the state of mind. The next chapter will be devoted to a study of these psychological sources of variation.

CHAPTER 8

Psychological Sources of Variation

Running like a red thread throughout all literature on time study is the steady emphasis on the necessity of standardizing the job pattern before attempting to take a time study.

The question that must be answered is: To what extent can the motion pattern of a job be standardized? Some enthusiasts since Gilbreth's day have gone even further in breaking down jobs into elemental motions than did the original masters.

For example, Captain Olsen of the Springfield Armory observes that with Taylor a new concept was born—the elemental operation.[1]

> Like an element in chemistry, this was considered to be the finest breakdown possible. However, within a few years, Frank Gilbreth had further subdivided the elemental operation into fundamental motions, which he called therbligs. Again these were considered the finest possible subdivision. Yet, just as the chemist has found the elemental atom subdivided into protons and electrons, . . . so the industrial engineer has seen his therbligs further subdivided into "movements," which . . . may be defined as muscular reaction required to move a part of the body.[2]

This is a beautiful analogy, but is it true? Can subdivided motions like these be reproduced in a perfect motion pattern by all individuals? The assumption that they can seems to challenge

[1] R. A. Olsen, "Setting Time Standards Without a Stop Watch," *Factory Management and Maintenance,* Vol. CIV, February 1946, p. 95.
[2] *Ibid.*

the whole concept of individual differences upon which modern industrial psychology is founded.

Olsen's point of view, that of the typical engineer, may be contrasted with the other extreme represented by E. Farmer, the British psychologist, who asserted that "the actual method finally adopted by the worker must be the one which he finds the most convenient; that is, the one best suited to his physical and psychological makeup."[3]

Uhrbrock opens his study of the psychological interpretation of wage incentive methods by indicating how his first flush of enthusiasm for motion-study research was shaken by Clarence Yoakum Stone's quizzical look at a University of Chicago seminar as he inquired if any method, no matter how good, could be considered the one best method.

This conflict in ideas between the thinking of the industrial psychologists, which is typified by Farmer's statement, and that of the industrial engineers, which is represented by Olsen's remarks, is not confined by any means to a disagreement between two professions. They represent several schools of thinking in psychology itself. After all, the engineers are merely continuing in the tradition of psychological atomism. This is essentially the kind of thinking established in experimental psychology by Wundt in Germany and carried to America by Titchener.[4] Despite the fact that Titchener was completely uninterested in applied psychology and considered it a second-class vulgarization of pure psychology, nevertheless it is his doctrine of psychological atomism that provides the basic assumption behind job breakdowns. Heidbreder expresses the fundamental Titchener doctrine as follows:

> The fundamental assumption is that in some way the more complex states of consciousness are always made up of elementary processes, that different combinations of elements, brought together in different degrees of intimacy, constitute . . . conscious experiences. . . .[5]

[3] Richard Stephen Uhrbrock, *A Psychologist Looks at Wage Incentives* (New York: American Management Association, 1935), p. 3.

[4] Edna Heidbreder, *Seven Psychologies* (New York: Appleton-Century-Crofts, Inc., 1933), p. 123.

[5] *Ibid.*, p. 138.

This statement would support Taylor's directions, which we have quoted earlier, on how to break down a job into its elementary motions and resynthesize these motions into a new job on the assumption that these elements compose an additive set. However, since Titchener was not interested in any applications of psychology, his thoughts stopped with the theoretical analysis.

John Stuart Mill, among the prescientific associationists who likewise were psychological atomists, at least never represented psychological phenomena as an additive set. Mill pointed out that complex ideas are not always matters of composition; they may be generated or result from simple ideas in very much the same manner that two chemical elements may form a compound with an altogether different set of properties from the elements from which they were formed.[6]

By way of contrast, Dewey observed that there were no partitions between the activities of organisms; when an act displayed unity, that unity was functional: it had no existential separateness.[7]

If Dewey's interpretation is correct, the psychological distinctions that the engineers use, such as therbligs, are then distinctions of their own making; they are not grounded in the nature of things but are convenient devices for *some* practical purposes. Whether or not they are as useful as the chemical atomic concept of Dalton, who was able to explain the law of multiple proportions with it, is yet to be demonstrated.

Similarly, the Gestaltists have attempted to demonstrate that psychological phenomena cannot be explained in terms of elements and their combinations. In an effort to demonstrate that in psychology the total is not necessarily the sum of its parts, Kurt Lewin has gone so far as to apply topological mathematics to psychology by demonstrating that a person is but a differentiated region in a topological life space.[8]

[6] *Ibid.*, p. 56.
[7] *Ibid.*, pp. 211–12.
[8] Kurt Lewin, *Principles of Topological Psychology* (New York: McGraw-Hill Book Co., 1936).

Strangely enough, the first applications of psychology to human problems were made by those industrial psychologists who revolted against the idea of psychological atomism, the functionalists at the University of Chicago under Dewey and James Rowland Angell in America and the Gestaltists in Germany under the leadership of Koffka and Kohler. This probably accounts for some of the severity of the conflict between the thinking of the industrial psychologists and that of the leaders of the industrial engineers, particularly in the early twenties.

Thus we have this strange contradiction: The school of psychology whose basic assumptions support most strongly the reasoning behind motion synthesis was completely uninterested in applications of the phenomenon; the schools of psychology whose assumptions would seriously undermine the idea of job standardization and job synthesis are the ones largely responsible for the many applications of psychology to everyday problems.

Hugo Münsterberg of Harvard, who, along with J. McKeen Catell of Columbia, is generally credited with founding the field of industrial psychology, concentrated attention upon the field of individual differences.[9] Most workers in this field have continued in his tradition; so that a point of view has developed somewhere between the extreme views of the Gestaltists, whose views would completely reject synthesized standards, and the industrial engineers, who, dissatisfied with mere motion molecules, are now seeking motion electrons and protons. Thus the problem of standardizing the job for time study purposes becomes: To what extent can the operations of the man-machine system be atomized, and the job resynthesized and standardized for use by every individual?

Myers of Cambridge, who has been the leading figure in English industrial psychology, is of the following opinion:

> Taylor's endeavor to establish 'rigid rules for each motion of every man,' followed by Gilbreth's 'Quest of The One Best Way,' . . . is in

[9] Morris S. Viteles, *Industrial Psychology* (New York: W. W. Norton & Co., 1932), p. 41.

diametrical opposition to the attitude of British workmen. Undoubtedly their intuitive opposition can be shown to have a sound psychological basis. For the mental and bodily differences between workers are such that it is impossible to train, or to expect, each worker to perform the same operation in identically the same way. In all sport and in all forms of art, there are different styles, all equally good, some suited to some men, others to others. So it must be in regard to industrial work. There *is* no 'one best way.' What the psychologist and physiologist insist is that there are undoubtedly bad styles and bad habits of work which the worker needs to be taught to avoid . . . Gilbreth's notion of 'the one best way' is . . . impossible strictly to carry out in practice, because no two workers can be trained to precisely the same features of rhythm and movement . . .[10]

Myers goes on to point out:

In those early days, too, it was supposed that success could be attained by dividing an operation into a number of different parts, in observing and standardizing the movements of a certain operative who performs one of those parts in the quickest time, in observing and standardizing the movements of another operative who performs another of those parts in the quickest time, and so on; finally collecting and stereotyping the different parts thus studied into one whole which is thereupon to be forced upon the worker as 'the one best way.'[11]

Condemning this practice, Myers observes that an individual is indivisible and that this procedure is no more effective than would be an attempt to build a new person by removing bodily organs where superior ones are discovered in another person and transplanting the better organs to the original person. The whole sense of pattern is lost.

It would appear that in standardizing a job we should not be governed by the therblig structure of the method, but recognize that different methods differ for different people within certain general limitations, laid down as a series of motion principles by Myers. If a worker's movements are analyzed, it is found that they fall into three classes:

1. Those strictly necessary for the work;
2. Those due to the worker's adaptation to rhythm of movement;

[10] Charles S. Myers, *Industrial Psychology* (London: People's Institute Publishing Co., 1925), p. 27.
[11] *Ibid.*, p. 81.

3. Those due to inexperience and poor workplace arrangement.

It is quite obvious that Class 2 will lead to variations in method as individuals differ.

Without any examination of the principles of motion economy that Myers proposes for the correction of Class 3, we may conclude that the standardization of methods in Class 2 represents the first great threat to the concept of a constant chance cause system creating a common population for the performance of groups of people. It does not interfere with our ability to predict future performance of the idealized individual in the idealized factory.

However, this question of predictability of an individual's performance is strongly influenced by the insistence of engineers that the performance must be time studied on the assumption that the individual always performs the job exactly the same way. This raises the whole question of the psychology of monotony. To what extent does a change in the motion pattern relieve the worker of the cumulative strain imposed by this monotony? None of the literature that has been published on monotony seems to treat this topic. Monotony is treated as a purely subjective phenomenon that is a function of an individual's make-up. Under the circumstances, the reply of the engineer in all likelihood would be to select a worker who does not require a change in work pattern to relieve monotony. Inasmuch as we have postulated an ideal man-machine situation in an ideal factory, we can dispose of this problem at this point, but recall that in a normal industrial environment, the purpose of time study is to set standards among the workers found on the job.[12] However, even under the idealized environment, the fundamental nature of the person prevents us from imposing any preconceived microscopic motion pattern upon the worker's job habits.

All studies on the effect of varying the pattern to relieve

[12] J. E. Barmack, "Boredom and Other Factors in the Physiology of Mental Effort," *The Archives of Psychology*, No. 218, 1937.

monotony have consisted of doing a completely different operation, such as folding different handkerchief styles. These studies never consisted of changing the work pattern on the same style.[13] It becomes apparent at once that the psychological influences of a mechanical nature, associated with a standard method of performing a job, remain undetermined in their effects on the constant chance cause system. We shall have occasion to examine again these concepts of standard methods and atomistic analysis of methods when we discuss the industrial practice of standardizing and improving jobs (Chapter 11) and the industrial practice of setting rates from standard data (Chapter 15).

Myers, in upholding the approach of the industrial psychologist to methods problems as against the approach of the industrial engineer, comments that "sometimes the mere presence of the Institute's [National Institute of Industrial Psychology] investigators and the interest which they have shown in the employees' work have served to send up output before any actual changes have been introduced."[14] This evidence leads us at once to a consideration of the purely emotional motivating influences affecting the constant chance cause system we have postulated.

The field of psychosomatic medicine has opened completely unsuspected areas of correlation between the emotions and bodily status.[15]

The Medical Research Council of Great Britain published a study entitled, "The Incidence of Neurosis among Factory Workers." They defined neurosis as including "only those disorders which were clearly illnesses both in the doctor's and in the patient's opinion, and which were clearly neurotic in nature

[13] S. Wyatt and J. A. Fraser, *The Comparative Effects of Variety and Uniformity in Work*, Industrial Fatigue Research Board, Report 52 (London: His Majesty's Stationery Office, 1928).

[14] Myers, *op. cit.*, p. 28.

[15] H. Flanders Dunbar, *Emotional Bodily Changes* (3d ed.; New York: Columbia University Press, 1946).

and also disabling—nearly always they had caused absence from work."

The writers estimated that "over 25 per cent of all sickness absence was due to neurosis, and ten per cent of the workers examined had suffered from disabling neurosis during the six months under review."[16] What makes these figures so significant is the early study of Halliday[17] on the changing nature of the illnesses of our working population. He traces the growing incidence of psychosomatic disorders and is inclined to contribute a good portion of them to the growing emotional frustrations attending present day work.

Young, in observing that the arousal of behavior necessarily implies a release of physical energy from the tissues, defines motivational psychology ". . . as the study of all conditions which arouse and regulate the behavior of organisms."[18] With the study of this motivating factor we shall complete our examination of the sources of psychological variation.

Taylor was very well aware of the importance of this factor. In fact, this factor provided the stumbling block which prevented the realization of much of his hopes for scientific management.

Much of his testimony before the Congressional committee investigating efficiency systems treats of his efforts to persuade hostile groups of workers that his technique of time study was in their long-run interest. Running through his testimony, for example, are remarks like this: "Workmen throughout the civilized world are firmly convinced that it is for their interest to go slow instead of going fast. That is, I conceive, the saddest, the most unfortunate fact in industry today."[19]

[16] Quotations from Russell Fraser, *The Incidence of Neurosis among Factory Workers,* Research Council, Industrial Health Research Board Report No. 90 (London: His Majesty's Stationery Office, 1947), pp. 1–2.

[17] James L. Halliday, *Psychological Medicine* (New York: W. W. Norton & Co., 1948).

[18] P. T. Young, *Motivation of Behavior* (New York: John Wiley & Sons, 1936).

[19] *Industrial Relations,* final report and testimony submitted to Congress by the Commission on Industrial Relations, created by the act of August 23, 1912, Vol. I (Washington: U. S. Government Printing Office, 1916).

The whole problem revolves around worker motivations in the modern industrial environment. How to free this motivating force has been the principal problem of the engineers. It is in an effort to understand this motivating force that sociological factors of the industrial environment must eventually be introduced to make up our fourth source of change in the constant chance cause system. Could the engineers succeed in releasing this motivating force completely, then time study for rate-setting purposes would be a very simple problem. The motivating force would be the driver; the critical ketosteroid output, the physiological ceiling limitation beyond which the motivating force would be restrained from going. The time study at this level of performance then would be a perfect sample of a constant chance cause system and from its arithmetic mean and accompanying standard deviation we should be able to set an objective time study rate with its accompanying variations. In the absence of any device to free this motivating force, sample time studies become meaningless as an interpretation of a parent population of data. The remainder of this chapter will be devoted to a study of the nature of motivation in its effect upon the constant chance cause system.

It might be interjected at this point that if conditions surrounding a time study indicate that a worker is stalling, that is, working at a conscious decreased motivating drive, the only logical conclusion that an engineer can draw from the sample study under such circumstances is that the study is useless. Any attempt to correct data for this effect is mere guesswork.

The importance of the emotional factor is pointed up by the day-to-day experiences of any union industrial engineer charged with the administration of a wage incentive payment program. The very elaborate plans described by Louden and Lytle all have one objective, and that is the release of this motivating factor to secure unrestrained production. It is but seldom realized that, aside from the fact that the motivating force released by the financial incentive is limited in degree, within a short

time after its introduction it becomes relatively ineffective.

The thinking of management on wage incentive payment plans generally follows this pattern. The job evaluation structure fixes a base wage. The rate is then presumably set to afford a worker an increment beyond this base wage. The worker is informed that once the rate is fixed it cannot be changed. Now, actually, management has a pretty specific idea of what they expect a man to earn beyond the base rate. They must interest themselves in this problem, all statements to the contrary notwithstanding; otherwise the variation in incentive increments may become so excessive that the basic job evaluation structure of the factory is upset, and with it the whole social hierarchy. This concept of the factory as a social system will be examined when we study sociological sources of variation.

The result always is that, though the rate for a specific job is never cut, if the incentive increment is found to exceed what the factory manager considers reasonable bounds, the next job is tightly rated and the average hourly wage of the man goes down. Now this presents no problem to the employer. He is still thinking of a man's wages in terms of a salary plus a bonus. The worker, on the other hand, seldom makes this mental separation; he thinks in terms of average hourly earning opportunities. All of his living habits have become adjusted to the increased rate of pay. With the tighter leveling, that rate of pay has been reduced. The worker very speedily discovers that what is more important to the employer than unrestrained productivity is the preservation of the basic evaluation structure.

Conversations with the leaders of the Pittsburgh C. I. O. on March 28, 1946, disclosed that an incentive situation in a large plant in Pittsburgh, similar to that described above, held up settlement of a strike. The company was attempting to tighten up its rate structure after the war was over and the workers were resisting. Thus the wage incentive plan that is initially installed to release this motivating drive very often clamps a tighter ceiling upon this motivator than existed before the

installation of the plan.[20] At least enough is apparent in this brief outline to establish the relative ineffectiveness of wage incentive payment plans in fully liberating the worker's production drive. The vicious circle to which it leads is highly paradoxical. Rates are uneven because we cannot predict the motivator possibilities of a worker through time studies. Because rates are uneven, wage incentive plans fail to liberate this motivating drive. All that can be said about wage incentive payment plans is that they tend to liberate more drive than straight day work does. It still leaves untapped a good reservoir of productivity. Here is a major challenge to the stability of any constant cause system.

Other emotional factors have an unpredictable influence on the motivator drive, among them the emotional cycle. Hersey found, as the result of a study of twelve men for a year, that there was a definite periodicity to their emotional tonus. It could not be accounted for by environmental happenings, climatic changes, or physical conditions, but it definitely affected the feeling of effort and performance on the job.[21]

It is important to remember that these psychological forces are just as real in the work situation as any physiological limitation. They cannot be dismissed as pure emotional bunk as so many practitioners have attempted to do. They in all likelihood represent the major problem in attempting to establish a constant chance cause system. They seem to occupy that area whose measurement is best represented by the border in the measurement diagram between Areas I and II (Fig. 1, p. 48).

Our picture of these emotional forces however does not end here. Thus far we have been examining the individual worker

[20] For complete development of this idea, see: William Gomberg, "Union Interest in Engineering Techniques," *Harvard Business Review,* Vol. XXIV, Spring 1946, pp. 356–365. Other aspects of the relative ineffectiveness of wage incentive payment plans may be discovered in: Van Dusen Kennedy, *Union Policy and Incentive Wage Methods* (New York: Columbia University Press, 1945).

[21] R. B. Hersey, "Rate of Production and Emotional States," *Personnel Journal,* Vol. X, 1932, pp. 355–364.

as a working animal exclusively. Now we shall pass to an examination of the complete individual in an effort to determine whether we can predict, even in our idealized factory, the effect of the various sociological forces upon this worker, with their consequent reaction upon his motivating drive toward higher production.

CHAPTER 9

Sociological Sources of Variation

Sorokin, the eminent Harvard sociologist, and his assistant, Clarence Berger, undertook a study of the specific activities making up the whole of human behavior. Dividing the living day of 24 hours into classes of activities, they discovered that human activities distribute themselves as outlined in Table III.

TABLE III

OVERT ACTIVITIES*

Activity	Average Time Spent per Member of the Total Group Daily†	Per cent of Participation by Members of the Group
Physiological needs	11 hr. 12.7 min.	About 100%
Economic	7 hr. 3.0 min.	2%—85%
Societal	1 hr. 20.9 min.	1%—55%
Religious	8.3 min.	20%
Intellectual	1 hr. 24.8 min.	2%—50%
Artistic	24.9 min.	1%—2 %
Love and courting	8.6 min.	6 %
Pleasurable	1 hr. 30.6 min.	1%—29%

* P. Sorokin and C. Berger, *Time Budgets of Human Behavior* (Vol. II, Harvard Sociological Studies; Cambridge: Harvard University Press, 1939), p. 76. Reproduced by permission of the publisher.

† The total of these classes of activities accounts for 23 hours and 13.8 minutes of every twenty-four hours. The remaining 46.2 minutes are spent in various activities, each of them lasting less than five minutes.

Now quite obviously the complete individual, even the idealized individual, will be strongly influenced in his work drive by

the degree of conformity of his individual behavior to this average distribution of the ways of spending leisure time.

Sorokin further found that out of a total of 1,917,225 minutes spent in all activities by the group under study, 286,582 minutes, or 13.85 per cent were motivated by force of circumstances;[1] that is, they were doing things under compulsion. They were doing things because they had no alternative. As would be expected, the two investigators observed that the daily work of the male was the activity in which this motive of force of circumstances was most dominant. It seemed to occupy the largest portion of the area of human frustration.

The negative attitude toward work has been analyzed in a group of workers on mass production jobs whose pattern lends itself ideally to time study. Walker and Guest have listed the characteristics of these jobs as follows:

1. Mechanical pacing of work.
2. Repetitiveness.
3. Minimum skill requirement.
4. Predetermination in the use of tools and techniques.
5. Minute subdivision of product worked on.
6. Surface mental attention.

All of these characteristics would lend stability to any constant or variable chance cause system. However, when we investigate the worker's reactions to these characteristics we get a negative response.

Walker and Guest observe that "a large majority of line workers voted immediate job content as their number one reason for not liking their jobs." They go on to add that the mechanical pacing of the work was the most disliked of all of these mass production characteristics.[2]

On the other hand, workers whose jobs offered some degree

[1] P. Sorokin and C. Berger, *Time Budgets of Human Behavior* (Vol. II, Harvard Sociological Studies; Cambridge: Harvard University Press, 1939), pp. 123–124.

[2] C. Walker and R. Guest, *The Man on the Assembly Line* (Cambridge: Harvard University Press, 1952), p. 142.

of variety, that is, repairmen and utility men, specifically pointed to the nonrepetitive characteristics of their jobs as reasons for liking their jobs.

Some mass production workers performed more than one job, and the investigators found a strong correlation between the intensity of liking the job and the total number of jobs assigned.

A favorite device which workers used to protect themselves against pacing was to build a bank of reserve subassemblies on the line to permit them to vary their pacing. In short, the very destruction of the statistical stability of the system made the work attractive.

Taylor's approach to this problem would be very simple. He would eliminate those men whose make-up did not conform to the emotionless stereotype required by his technique. He instructed management that a cardinal principle of scientific management is selection of the proper workingman for the job.[3] This stratification of the working sample would solve the problem by eliminating it. However, we have to deal, in actual practice, with the working population as we find it. We are extremely restricted in our selection.

The reader is reminded again, however, that we have postulated an ideal worker in an ideal environment. Therefore, we can conclude that this factor does not influence the constant chance cause system governing the productivity of the individual whom we have postulated. It is assumed that the ideal worker is pleased with the work he is performing. In short, Taylor's directions may be followed under these artificial conditions. Viteles confirms these findings. Drawing upon DeMan's work he states that only 11 per cent of unskilled workers find any joy in their work, as compared to 44 per cent of those who were semiskilled and 67 per cent of those who were skilled.[4] However, in an effort to refute the findings of the Western Electric experiment, he quotes the statement of Kornhauser

[3] Frederick Winslow Taylor, *Principles of Scientific Management* (New York: Harper & Brothers, 1911), p. 36.

[4] Morris S. Viteles, *Industrial Psychology* (New York: W. W. Norton & Co., 1932), p. 567.

and Sharp that the *"efficiency ratings of employees showed no relationship to their attitudes."*[5]

Offhand this would tend to sustain the theory that the constant chance cause system is independent of not only sociological pressure but also individual psychological influences. Actually, such is not the case at all. The odds are that the variation in the force of circumstances making these people go to work in the first place affected their relative efficiency. This, of course, would make their productivity an unpredictable variable. In short, it would dominate the constant chance cause system.

Evidence that sociological factors dominate the working level of groups of workers is offered by the findings of the Western Electric experiment to which we have continually referred. The essence of this experiment may be summed up by describing the findings of the workings of the factory as a social system.

Mayo expressed the entire spirit of the Western Electric inquiry when he observed that after it had been found that production increased after the installation of rest periods, it would have been a simple thing for Pennock, supervisor of the experiment, to have shut down the inquiry after announcing the fact that the introduction of rest pauses increased production was confirmed by this latest series of experiments. It became obvious, however, when the increase in production was maintained after the removal of rest periods, that some of the superficial conclusions about the correlation between changes in mechanical working conditions and production variations required a deeper explanation.[6]

Mayo, in commenting upon his own work on rest pauses in a textile mill in 1924, points out that he had to revise his own conclusion some twelve years later about the reasons for the improvement in productivity. Referring to this old set of observations, he remarks that:

At the time when we completed our part in the work, we were sure that we had not wholly discovered the causes of the high labor turnover. We

[5] *Ibid.*, p. 577.

[6] F. J. Roethlisberger and William J. Dickson, *Management and the Worker* (Cambridge: Harvard University Press, 1939), pp. x–xi.

could not even attribute the change to the mere introduction of rest periods; many other changes had been simultaneously introduced. For example, we had listened carefully and with full attention to anything a worker wished to say.[7]

The importance of the sociological factors in influencing the factory production norm has been summarized by Roethlisberger and Dickson. Commenting upon the results secured in the Relay Assembly Test Room, they observe:

> It is apparent that the logic of a controlled experiment was responsible for many strange occurrences in the test room. . . . In the endeavor to keep the major variables in the situation constant and the girls' attitudes cooperative, the investigators inadvertently altered the total social situation of the group. Thus, as a consequence of setting up an experiment to study the factors determining the efficiency of the worker, they abrogated most of the rules intended to promote and maintain efficiency. The "rules of the game," as they ordinarily applied in the shop, were changed.
>
> . . . the investigators had not been studying an ordinary shop situation but a socially contrived situation of their own making. . . . In the process of setting conditions for the test they had altered completely the social situation of the operators and their customary attitudes and interpersonal relations. . . . This situation included not only the external events but the meanings which individuals assigned to them. . . . As much attention had to be given to these psychological factors as to output in assessing the external changes which took place.
>
> It had been evident in the effects of the experimentally introduced changes in working conditions, which had proved to be carriers of social meaning rather than mere changes in physical circumstances. It had shown itself in the output variations of certain operators, which could be related to their personal preoccupations, and which continued as long as these preoccupations persisted.[8]

All attempts to relate the experimentally introduced changes to the operator's performance, apart from their effect upon the operator's attitudes, had been inconclusive.

In an effort to learn more about the nature of these attitudes

[7] Elton Mayo, *The Social Problems of an Industrial Civilization* (Boston: Division of Research, Graduate School of Business Administration, Harvard University, 1945), p. 66.

[8] Roethlisberger and Dickson, *op. cit.*, pp. 182–184.

which seemed so intimately related to production levels, an elaborate interviewing problem was undertaken from which the two investigators drew the following conclusions.

People working in similar surroundings did not react in the same way to those surroundings. Some expressed satisfaction, some dissatisfaction with similar plant conditions, wages, and working conditions.

It became clear that many employee comments which had formerly been interpreted in terms of the interviewee's personal situation could be better understood if they were interpreted in the light of the employee's existing social relations within the plant: the social organization of the group with which he worked and his position in that group.[9]

According to this interpretation, it is not possible, as in the more abstract social sciences, to treat material goods, physical events, wages, and hours of work as things in themselves, subject to their own laws. Instead they must be interpreted as carriers of social value.

It becomes at once apparent that the sociological impact of the workers' social codes in the factory is a major factor in determining to what extent the motivating drive will be liberated in order to reach the production limit fixed by the physiological ceiling which we have discussed.

Roethlisberger found that every factory social organization could be divided into two major divisions, the formal blueprint organization which prescribes the cold formal relationships between people in the carrying out of their functions in the industrial enterprise, and, equally important, the informal organization dictating the quality of the social relationships between the members of the firm and the full extent of the co-operation that is actually realized. This informal organization compels adherence to its mores by a host of forces that range from physical assault to biting sarcasm and social exclusion from the group.

The assumption is made by management that workers are completely economic beings whose sole motivation is dictated

[9] *Ibid.*, pp. 373–374.

by the logics of cost and efficiency which the management executives attempt to satisfy with wage incentive payment plans; actually worker behavior is dictated by the informal rules of the social organization. It is only comparatively recently, since the completion of the Western Electric experiment, that these logics of sentiments have received formal expression in collective bargaining negotiations. One of these sentiments is a group acceptance of what constitutes a fair day's work.

The tendency of the time study technician to dismiss workers' prejudices, as he would call them, is senseless. It generally results in a conflict. Strangely enough, in many cases the conviction of a group of workers that they are performing a full day's work is the very factor that makes the job a normal day's work. To attempt to ignore this psychological force, which has its origins in the group acceptance, in the name of a scientific approach to measuring a fair day's work makes no sense. It leaves the engineer without any criterion against which to match his measurements except, perhaps, what is happening in another plant. In another plant there may be an entirely different interplay of social forces at work. Such a procedure, far from being a scientific one, actually constitutes a cardinal violation of the scientific method which is pointed up by Rautenstrauch. He observes, "Any procedure in planning which neglects the human emotions is unscientific because the scientific method demands the consideration of all the factors affecting a given situation."[10]

Mayo applies this very specific principle to rate setting with the following terse comments:

> The efficiency experts had . . . regarded workers' statements as exaggerated or due to misconception of the facts and, therefore, to be ignored. Yet to ignore an important system—whatever its character—on supposedly moral grounds is preposterous.[11]

Most human relations experts hope to restore managerial control of this situation by manipulative means that will flow from

[10] Walter Rautenstrauch, "The Scientific Method in Human Affairs," *American Scholar Forum*, Fall 1945, pp. 475–493.

[11] Mayo, *The Human Problems of an Industrial Civilization*, p. 67.

this new understanding of the dynamics of human behavior.[12] There have been no end of new techniques proposed to this end.

However, the very process of manipulation on live human beings elicits a destabilizing reaction. The educational department of the United Automobile Workers Union published a deeply caustic article in their educational department publication, *Ammunition.* It is called "Deep Therapy on the Assembly Line." The writer sarcastically wants to know when the National Labor Relations Board will make it an unfair labor practice to let a psychiatrist or a "cow sociologist" loose on assembly line captives.

A new group of industrial social psychologists have attempted to combine some of the findings of Roethlisberger and Mayo, the topological psychology of Kurt Lewin, and some of the insights into interpersonal psychiatry into a set of human relations techniques technically known as group dynamics that will liberate the workers' motivation drive completely.

We might ask ourselves the question: Will this new technique liberate the complete motivating drive of the workers? Can these techniques be used to approach or approximate the completely motivated workers of our idealized factory social system?

Norman R. F. Maier describes the purposes and methods of these techniques so lucidly that many of their contradictions reveal themselves at once. Maier pleads for a change from autocratic management to democratic management. He feels that decisions made by a group are much more likely to be carried out enthusiastically than decisions imposed upon a group by a leader.

He describes two experiments from the needle trades to make his point. In the first of these factories, he describes an experiment conducted by Bavelas and reported by Lewin. Lewin was a world figure in the field of child pyschology but freely

[12] See Harold Sheppard, "Managerial Sociology," unpublished doctoral dissertation, University of Wisconsin, 1948; Daniel Bell, "Adjusting Men to Machines," *Commentary,* January 1947; and William Koivisto, "The Value Orientation in Selected Industrial Relations Studies," unpublished doctoral dissertation, The University of Chicago, 1951.

confessed his ignorance of industrial practice. The factory was nonunion. Bavelas conducted a meeting of the girls, and it is reported that the girls unanimously agreed that they would like to set their goal at 84 units per hour. Within five days this goal was exceeded. Another meeting was held and this time the girls, unanimously again, of course, agreed that they would set a goal of 95 units per hour. It is then reported that the production stabilized itself somewhere around 88 units per hour. Before these meetings had been undertaken the average girl produced in the neighborhood of 74 units per hour.

I am just curious to know how free any individual girl felt to say in this nonunion factory: "Look, these are man-paced jobs on which we are working. I feel that the employer is getting a fair days' work out of us and ought to be satisfied."

This type of democracy, no matter how skillfully administered by a competent technician like Bavelas, must degenerate into manipulation. This smacks of "Stakhanovite" democracy in which good loyal Soviet workers voluntarily pledge their all for the workers' republic.

Another experiment is described, a union factory this time, by Coch and French. This factory is in contractual relations with the International Ladies' Garment Workers' Union. The factory at the present time is bedeviled by at least as many piece rate disputes as any ordinary factory. The results, if any positive ones were ever obtained, ceased when the workers discovered that they were not accompanied by any corresponding economic reward. The account reads like a patent medicine testimonial by a victim of a dread disease who secured a temporary emotional relief from some transitional symptoms. The testimonial appears long after the patient is dead and buried.

The weakness of these techniques does not lie in the methods themselves but in the value system in which they are applied. When we are told that "Frequently the 'how to do a job' rather than the 'what to do' is the problem that can be solved,"[13] "we

[13] Norman R. F. Maier, *Principles of Human Relations* (New York: John Wiley & Sons, Inc., 1952), p. 1.

are apt to dismiss the technique as at best palliative and superficial." When, however, the very first paragraph which is designed to sell the technique to management states that "the leadership of management feels itself to be in competition with the union leadership for the loyalty of its employees,"[14] we become downright suspicious.

Such an observation should logically lead to a recommendation that management undertake self-psychotherapy rather than attempt to manipulate the other parties in the picture.

The techniques of and by themselves remain neutral and can be used for valuable purposes. The writer, in fact, was negotiating with Kurt Lewin to employ his services in areas where the union was plagued with racial tensions. This would constitute a constructive use of group dynamics in contrast with what has been outlined here.

There can be no question that group dynamics is an effective technique that can be used to encourage group participation and thereby raise the motivating drive of workers. Statisticians would call this changing the nature of the underlying population. It would lead to a chance cause system stability at a higher level. However, the misuse of the technique will elicit the very reverse of this effect. Democracy is not play acting in the castle of a feudal lord, even when the castle is a factory. Group dynamics can take place successfully in the presence of real democracy where both the employer and employee have the power to express what is really in their minds.

Maier is aware that the person who applies group dynamics is more important than the technique itself. He writes for example:

> On the other hand, a supervisor who has an honest interest in people will, without training, do many things that reflect this interest, and he may make many mistakes in technique without having his true attitude toward employees misunderstood. . . . Words and actions communicate content and feelings, and the feelings that are communicated greatly depend upon the attitude of the person who expressed the words and actions.[15]

[14] *Ibid.*, p. 11.
[15] *Ibid.*, p. 4.

However, again we resort to the fiction of the idealized worker who is independent of the pushes and pulls of these techniques and social forces. We have protected our idealized person in the idealized situation by assuming that the amicable relationships developed in the factory have permitted full liberation of the motivating drive and that the effect of these sociological factors is inapplicable.

In assessing the importance of various forces in their effect upon production, Whitehead concludes, from the concept of the factory as a social system affecting the productivity level:

1. The parts of the system stand to one another in the relation of mutual dependence.
2. The parts of the system show differences in kind. These parts exhibit an organic unity by their unique contributions to a common process.
3. The system possesses stability. The members so acted as to tend to maintain the process. After a small interference, the process tended to approach the condition it would have reached but for that interference.[16]

In addition to the sociological force of the factory as a social system, the sociological forces within society at large shape the working attitude of most workers. We find Whitehead noting that the productivity rate among the relay test operators, which had been consistently increasing during the course of the experiment, fell off rather sharply toward its end. Recalling that this sharp decline coincided with the beginning of the depression in 1930, in explaining this phenomenon, Whitehead remarks:

Near the end of the experiment, the experts were informed that they would be laid off in a few weeks. This constituted a new situation with many implications. The operators were hard up, their families were out of work, and financial considerations might have suggested the advisability of working unusually hard for the remaining time. On the other hand, this news in effect told the girls that nothing they could possibly do would save their human relations from disruption both within the plant and to a certain extent outside of it. They adjusted themselves to the social situation and output fell dramatically; as one of the operators subsequently expressed it, "We lost our pride."[17]

[16] T. N. Whitehead, *The Industrial Worker* (Cambridge: Harvard University Press, 1938), I, p. 258.
[17] *Ibid.*, p. 243.

Again we may conclude that the control of the sociological system of the community and the factory over the psychological attitude of the worker will determine to what extent the motivating drive will be liberated. We assume arbitrarily in our idealized factory that none of these forces interferes with the liberation of this drive.

CHAPTER 10

Conclusion of Part One

A review of our basic time study concept reveals the following:

We indicated that if a time study is to rest on sound logical foundations as a means of predicting future performance from sample studies, then the individual man-machine system for which the prediction is being made must be in a state of statistical control.

We defined this state of statistical control in terms of a physical constant chance cause system and the quantitative characteristics of the sequence of observations produced by such a cause system. (See Chaps. 3 and 4.)

We then postulated an idealized person in an idealized environment which would approximate this physical cause system. From there we developed quantitative criteria to determine whether or not the system was in a state of statistical control. We then established tools to set a standard, the range of accuracy for the standard, and the number of readings required to secure the accuracy necessitated by collective bargaining negotiations. Next, we established the relative accuracy of the time-measuring instruments used in the time study.

Thenceforward we undertook a systematic a priori examination of the possible sources of variation which would be likely to influence the constant chance cause system postulated.

We discovered that the mechanical sources of variation could be controlled, or at least their effect could be predicted. Physiological sources of variation were somewhat elusive but the work of Hoagland and Pincus presaged the discovery of an objective measure of physiological tax. This measure would permit us to place a ceiling beyond which production should not be driven, once we learned more about the correlation between this measure and the incidence of illness, aging, and maximum life expectancy.

Within this ceiling we conceived of the psychological and sociological influences as governors of the motivating drive that could establish different normal work loads within a very wide range. The ceiling to this range would be the maximum safe physiological tax. It could be graphically represented as in Figure 8.

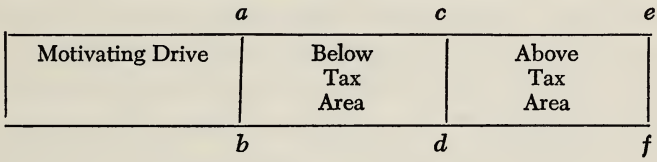


FIGURE 8

Our idealized person in an idealized environment postulated a state of statistical control vibrating about *cd*. Our workaday world, however, indicates a possible normal work load anywhere between *ab* and *ef*, where *abcd* represents the area of normal loads below the physical optimum and *cdef* represents the area above the physical optimum.

We are now ready to answer the original question which we postulated. Into what area of measurements do time study phenomena for rate-setting purposes belong in terms of predicting the future performance of individuals? We are not discussing the relative position of individuals with respect to one another, merely the predictability of individual performance from sample studies.

The answer is that in our artificially postulated environment the phenomena fit into Area II (Fig. 1, p. 48), the area occupied by statistical laws. It cannot be emphasized too strongly that in our present workaday world this normal load which depends upon the emotional set of the individual workers is a reasonably unpredictable variable. We have emphasized the reality of this economic force. Under the circumstances, the phenomena belong to the border area between Areas I and II. They are not subject to a constant chance cause system but to a variable chance cause system. This means that, even in terms of the predictability of future individual performance from a sample time study, most time study techniques lack a sound logical foundation. The prediction made under the best circumstances can only be an expression of a crude empirical hope.

The difficulties of time study for rate-setting purposes go far beyond the relatively simple problem of predicting future performance from the records of past performance of an individual worker. Assuming that this problem could be solved on a scientific basis, then another problem arises, that of relating the individual performance to a general concept of the normal. This problem will be investigated in the course of an examination of existing time study practice. Now that we have established the criteria for an objective time study system, it will be our endeavor to review these current techniques in an effort to evaluate the claims that are made for them. We have established the relative improbability of being able to predict an individual's future performance on the basis of past sample time studies. These conclusions have followed from the theoretical considerations about the conditions which a sound scientific technique must satisfy.

Sylvester has taken strong objection to these conclusions. He writes: "Gomberg has punctured the theory and practice of rating and leveling in so many places and with sufficient logic to make an alternative theory worthy of thoughtful examination." He then goes on to say that: "I believe that Gomberg is incorrect in placing time-motion study in the field of measurement that

lies on the borderline of his area of complete random variations and the area of statistical laws."

He adds that his work establishes "the position of time-motion study squarely in the field of statistical laws. If it approaches any borderline, it would be that of the area of exact physical laws."[1]

Davidson makes a rather exhaustive analysis of the techniques proposed by Sylvester and comes to the conclusion that they do not satisfy the criteria set by Sylvester himself.[2]

Abruzzi comes to a similar conclusion after he reviews the techniques. He writes: "Unfortunately the procedures that are recommended here have little statistical or empirical justification. Nor are they objective except in the superficial sense that the exercise of subjective judgment is in some cases transferred to the treatment of observed data."[3]

The most confusing recommendation made by Sylvester is that the time study analyst's choice of a statistic to summarize the observed data depends upon the analyst's subjective estimate of the operator's skill and motivation. We may observe that Sylvester has made a bold attempt to bring statistical thinking to operational time study. However, our original conclusions must still stand; Sylvester has certainly not demonstrated that time study data fits into the field of statistical laws—much less approaches the area of exact physical laws.

Davidson has, however, raised the question of the validity of the entire model proposed. He acknowledges that the question: "Is time study a science?" is a useful one, but he then goes on to ask whether or not this particular question can lead to any practical criterion for the selection and development of a useful methodology. There can be no disagreement with Davidson up to this point. Without the cloak of a scientific veneer the en-

[1] Arthur L. Sylvester, *The Handbook of Advanced Time-Motion Study* (New York: Funk and Wagnalls Co., 1950), pp. 5, 8.

[2] Harold O. Davidson, *Functions and Bases of Time Standards*, a research report (Columbus, Ohio: American Institute of Industrial Engineers, 1952).

[3] Book review by Adam Abruzzi of *The Handbook of Advanced Time-Motion Study, Industrial and Labor Relations Review*, Vol. 4, No. 2, January 1951, p. 307.

tire question of whether or not the setting of production standards is a management function or a joint function would never have arisen. There are any number of empirical arrangements between workers and employers governing the everyday process of rate setting. The result is an unending stream of products from which workers earn wages, manufacturers collect profits, and consumers take their wants. The process serves every pragmatic criterion offered by a dynamic economy. The procedure satisfies the sense of equity of all three parties to the arrangement, worker, employer, and consumer, or the process would come to a halt.

The question: "Is time study a science?" arises when we wish to analyze existing techniques in an effort to seek an improvement over what we are doing.

Davidson then goes on to say that "Gomberg's answer[4] to the question does define a theoretically ideal system which might be approached." However, later Davidson expresses some doubts on this matter.

Davidson cites the statement of the writer that some students of time study may object that in the name of science we may be transplanting the statistical view of physical reality instead of the mechanical dogma into the time study field. Davidson then adds parenthetically the statement: "We will indeed voice this objection and will substantiate it with both a priori reasoning and with experimental evidence, as well." Davidson has confused a theoretical ideal proposed by the author with a recommended operational economic procedure if sample time studies are to tell us anything about the underlying parent universe with a claim to picture the actual physical reality. As a matter of fact, Davidson's chapter entitled the "Appropriateness of the Constant Chance Cause System Concept to the Performance-time Phenomenon of Worker Paced Activities," reinforces the same conclusions reached on an a priori basis in this very chapter—that it is highly unlikely that worker performance can fit a constant chance cause system. It is for that reason that the

[4] Davidson, *op. cit.*, p. 276.

conclusion was reached that worker performance time measurement probably falls into the area between complete random variation and the area of statistical measurement.

The very practical conclusion to which Davidson is led by his rejection of any scientific model is one with which the writer is very unlikely to take any issue. He writes: "Joint labor management action in rate setting or rate revision does not correct any defects in the techniques which may be used, but it does tend toward greater acceptability of the results to employees."

Abruzzi completely accepts the implications of the application of Shewhart's philosophy to time study phenomena. He acknowledges that it is by no means certain that statistical procedures and criteria can be worked out for production problems. He then goes on to say in evident agreement: "But unless such procedures and criteria can be worked out, Gomberg warns, it will be extremely difficult, if not impossible, to develop production standards."[5]

The conclusions at which we have arrived here stem largely from an a priori examination of the small likelihood that no assignable cause of variation will arise in the four principal factors influencing the production rate, the mechanical factor, the physiological factor, the phsychological factor, and the sociological factor.

Abruzzi proposes an a posteriori method of examining actual distributions to test them for statistical stability. The distributions which he collated in the ladies' garment industry factories are subjected to tests for what he calls local stability and grand stability.

The distinction between local stability and grand stability is described by him in the following words: "In the case of local stability the production rates involved represent a continuous series of items produced during a period of several hours or during a complete shift. In the case of grand stability the production rates represent items produced over a protracted

[5] Adam Abruzzi, *Work Measurement* (New York: Columbia University Press, 1952), p. 35.

period."[6] We shall have occasion to examine these concepts in somewhat more detail in the next chapter.

We shall now review present-day industrial practices to detect:

1. Whether the procedures recommended introduce any new theoretical consideration which may upset our system;

2. To what extent the conclusions formulated about production standards can be considered acceptable. In addition, in Part Two we shall attempt to present an outline of collective bargaining techniques designed to enable industrial rate-setting processes to function with a minimum of friction.

[6] *Ibid.*, p. 37.

PART TWO

CHAPTER 11

Defining the Job

Present Practices, Time and Motion Study

We have already referred to the limited studies of American time study procedures made by Adam Abruzzi. Their limitations have already been delineated. It is nevertheless interesting to look at the way the factory management included in this limited sample described the criteria which they used to determine when an operation is ready for a time study.

Group Description[1]	Frequency
1. All obvious improvements have been made; principles of motion economy fully applied.	18
2. Operating procedure is fully applied, including methods, equipment, and materials.	16
3. Operation controlled with small variations; operation running smoothly and delays minimized.	12
4. Operation in close agreement with production standard; quality standards are being met.	16
5. Performance results; judgment of foreman.	4

The question which we must now answer is: To what extent do the existing techniques of industrial time study practice ap-

[1] Adam Abruzzi, "A Survey of American Time Study Procedures," *Time and Motion Study*, Vol. 2, No. 1 (R. A. Lister & Co., Ltd., January 1953), p. 16.

proximate a scientific pattern? We have already seen that the only justification for generalizing a production rate from a sample time study consists in a postulated constant chance cause system. We have seen that while the mechanical and physiological factors may be controlled, the psychological factor, particularly under the impact of sociological pressures leads to an unpredictable universe for all practical purposes. Hence from a strictly rational point of view the measurement of time study phenomena really falls into the borderline region between Areas I and II in Figure 1 (p. 48), somewhere between complete indeterminacy and the beginnings of statistical law.

The argument that a production standard works, that is, that the quota is met by a working man, is a limited criterion. We have already seen that there is a wide range of production quotas, all of which may be correct. This range is conditioned by the motivating factor or psychological drive on the one hand and the limit of the physiological capacity on the other. Thus it may very well be that a production quota set by a time study engineer is met because psychological drive pushes production to that point even if that point is beyond the worker's physiological capacity. This drive may be conditioned by fear of loss of the job, or any other of a number of factors. *The important thing to remember is that, because a quota is met, it does not follow that the rate is a valid rate.* Since Carroll's stricture that rate setting must be confined to the expert group can be justified only on the grounds that the time study practitioners have developed a scientific technique of time study, it would be profitable to review existing time practice with a view to evaluating this claim. Time study experts may have developed some aspect of reasoning that has perhaps escaped us, or that supplements our logical development. The analysis of the time study systems will consist of an examination of the standard texts that have provided industry's technicians with their methods, plus some specialized manuals by consultants who are operating within American industry. This analysis will be particularly valuable because in every case these manuals and texts represent the writers' ideas of ideal procedures which are seldom realized in

actual industrial practice. The analysis will seek to evaluate the treatment of the following topics:

1. Method of improving the job;
2. Method of standardizing the job;
3. Method of rating the individual performer during the study;
4. Correcting the data for special allowances.

At this time we shall not consider the technique of standard data. This will be discussed in the next chapter. The remainder of this chapter will concern itself with topics 1 and 2.

Again the reader is reminded that this analysis confines its attention to man-paced jobs independent of any automatic cycles. It should be quite clear by this time that the solution of the man-paced operation will automatically lead to the solution of machine-paced jobs by fixing limits to the speed of the automatic cycle.

Lowry, Maynard, and Stegemerten point up the necessity for analyzing the job and improving it before timing it so that it can be standardized. They state that "the purpose of doing this [motion study] is to improve methods and conditions as much as possible."[2] Their basic philosophy and approach to this technique is expressed in five laws and eight corollaries of motion economy. Numbers 3, 4, and 5 of the laws and Number 2 of the corollaries follow, inasmuch as these are the laws that challenge our special interest:

3. The motion sequence which employs the fewest basic divisions of accomplishment [therbligs][3] is the best for performing a given task.

[2] Stewart M. Lowry, Harold B. Maynard, and G. J. Stegemerten, *Time and Motion Study* (3d ed.; New York: McGraw-Hill Book Co., 1940), p. 30.

[3] A therblig (*Therblig* is Gilbreth's name spelled backward.) is an elemental hand motion defined by Gilbreth as a fundamental work element. All jobs can be classified into combinations of these elements. (Lowry, Maynard, and Stegemerten, *op. cit.*, p. 75.) Motions are divided into classes by Lowry, Maynard, and Stegemerten (*op. cit.*, p. 114), as follows:

1. Finger motions;
2. Finger and wrist motions;
3. Finger, wrist, and forearm motions;
4. Finger, wrist, forearm, and upper-arm motions;
5. Finger, wrist, forearm, upper-arm, and body motions.

4. When motions are confined to the lowest practical classifications, maximum performances and minimum fatigue are approached.

5. When conditions are the same, the time required to perform all basic divisions of accomplishment is constant for any given degree of skill and effort.

[Corollary] 2. The shortest time taken for each motion during the course of the study made on an expert operator should be considered the desired standard; all variations of time from this standard should be analyzed for each motion and the causes determined and recorded.[4]

Laws are generally proposed when the evidence for their truth is overwhelming. Is the evidence for these "laws" satisfactory enough to term them laws? It will be recalled from our chapter on the psychological sources of variation affecting the constant chance cause system that we observed that many industrial psychologists question the ability of every worker to pursue an identical motion pattern specified in terms as minute as therbligs. Other questions are whether or not therblig arrangements constitute an additive set or combine like chemical elements. In any case, if we assume that the psychologists are right and the engineers wrong, then Laws 3 and 4 may very well be wrong. For example, Laws 3 and 4 would seem to do away with the necessity for a baseball pitcher to wind up before hurling the ball to the batter. This wind-up employs more therbligs and involves higher practical classifications than either Law 3 or Law 4 would countenance. Workers perform many industrial tasks with certain extraneous motions that are evidently necessary to their basic make-up.

In commenting upon Law 5, the three authors remark:

If it were conceivable that an operator working at a given performance level would perform the same operation under the same conditions in a different length of time on one occasion than on another, then it would be impossible to establish definite standards.[5]

This is a tautology. Naturally if performance level is defined as speed of operation, the statement is true. However, the discovery that human performance under industrial conditions be-

[4] *Ibid.*, p. 96.
[5] *Ibid.*, pp. 102–103.

longs in that area of measurement on the borderline between indeterminacy and the beginnings of statistical control certainly does raise the question of how consistent human performance can be expected to be in making any time study a gauge of future performance.

In discussing Corollary 2, the authors remark:

> It is reasonable to assume that if an element can be done within a certain time once, it can be done within the same time again, provided the conditions are the same. Using the shortest time taken by an expert operator as a standard, an attempt should be made to ensure that this standard will be attained on all cycles.[6]

This statement is open to question unless we revert to the old mechanical dogma. Conditions are never quite the same. Even if we grant the truth of the mechanical assumption, the time study technician would have to be endowed with the Laplacian universal mind to keep those conditions constant.

Barnes divides what he calls the principles of motion economy into three broad classifications:

1. Use of the human body;
2. Arrangement of the work place;
3. Design of tools and equipment.

Under the first classification on the use of the human body, he states as Principle 4: "Hand motions should be confined to the lowest classification with which it is possible to perform the work satisfactorily."[7] This principle, however, is conditioned by other principles on momentum, the use of continuous curved motions, and the use of ballistic motions. Although this condition is hedged very carefully, nevertheless the same objections that were lodged against the Lowry-Maynard-Stegemerten Laws 3 and 4 seem to be applicable here. It is doubtful that the same series of hand motions is equally applicable for every individual. This matter has been treated at some length in Part One under the psychological sources of variation.

[6] *Ibid.*, p. 104.

[7] Ralph M. Barnes, *Motion and Time Study* (2d ed.; New York: John Wiley & Sons, 1940), p. 144.

Thus the very task of improving a job may result in some disputes about what is the one best method. Again we are confronted with the question: Is there a one best method equally applicable to all people?

There are no doubt some principles of motion economy that are equally applicable to everybody, particularly those that Barnes specifies for the arrangement of the work place and the design of tools and equipment. The problem that the time study technician faces in attempting to improve the job is to determine at what point of division the job breakdown becomes meaningless for changing its specifications; that is, to what extent can the detailed motion pattern be standardized? This question is of basic importance in assessing the value of rate-setting systems by the use of standard data.

In this connection, Barnes and Mundel have reached some vital conclusions. In studying the therblig breakdown of the time required to position pins in bushings with beveled holes, both investigators reached the conclusion that the standard time for certain therbligs cannot be given (even for a specific operation) as independent values since they may be influenced by other therbligs in the cycle.[8]

If this is true in as simple an operation as is herein described, it should be at once apparent how tenuous any conclusions must be about what constitutes the one best method. Although Lowry, Maynard, and Stegemerten recognize this difficulty in securing fixed motion times, they nevertheless conclude that these motion times, that is, therblig times, can be used for assessing the one best method.[9] This does not seem to make sense even on the assumption that these motion patterns are transferable to all people.

Some interesting light has been thrown upon this problem by investigators from other fields of psychology. Donders, the eminent Dutch physiologist, had hoped to get standard psycholog-

[8] Ralph M. Barnes and Marvin E. Mundel, *A Study in Simultaneous Hand Motions* (Studies in Engineering, Bulletin 17; Iowa City: University of Iowa, 1939).

[9] Lowry, Maynard, and Stegemerten, *op. cit.*, p. 134.

ical constants for reaction times. Here, too, the assumption was made that these reaction times constituted an additive set. The plan was this: A subject was to react whenever he saw a light flashed; this would constitute a basic reaction time. Next a red and green light would be flashed at different intervals; the subject was to react only when the red light was shown. The a priori reasoning was that the first reaction called for choice, the second for choice plus discrimination. Thus by making additional experiments and the proper subtractions, it would be a simple matter to fix specific reaction times and build a library of standard reaction times. No evidence was found to support this hypothesis. Lange did find, however, that reaction time varies directly with attitude, the factor which we have called our motivating drive.[10] It may very well be that these standard motion times are quite as unreal as the so-called psychological constants.

It is apparent, therefore, that it is a moot point at which stage of the job breakdown both job improvement and job standardization can take place for any group of people. Taylor started with job elements, Gilbreth reduced them to therbligs, Segur was the first to talk in terms of standard motion times, and then Holmes wrote in terms of muscular reactions. As we have seen, most industrial psychologists, finding no justification for Taylor's reshuffling of job elements, violently objected to Gilbreth's specification of standard therblig series. Of course, their answer to Segur has been the denial of his law of motion time that "within practical limits the times required by all expert workers to perform true fundamental motions are constant."[11] This does seem to fly in the face of all evidence about individual differences. Segur denies the existence of these differences.

Barnes and Lowry, Maynard, and Stegemerten have made more careful analyses of the implications of motion study for

[10] Edna Heidbreder, *Seven Psychologies* (New York: D. Appleton-Century Co., 1933), pp. 88–90.

[11] A. B. Segur, "Motion Time Analysis" [*1938*] *Proceedings of the National Time and Motion Study Clinic* (Chicago: Industrial Management Society, [n.d.]), p. 38.

rate setting through time study than any other investigators. The remainder of the students of the field have largely re-echoed their basic approach.

Merrick devotes practically no space to the problem of job improvement before standardizing the job.[12] He evidently was satisfied for the time study technician to follow Taylor's directions to which we have already made reference on pages 42–43.

Lichtner continues in very much the same tradition as Merrick. He devotes himself primarily to the necessity for standardizing physical conditions such as material and equipment. A few passing remarks about the psychology of working men suffice to treat standardization of the performance of the job.[13] Here again the implication seems to be that reliance was placed on the original rules laid down by Taylor.

Schutt devotes a chapter to the work of Gilbreth and his development of therbligs. No attempt, however, is made to develop arbitrarily best motion patterns. Schutt indicates his awareness of the fact that not only the motions themselves are important but the sequence within which they appear.[14] This is an evident reference to our observation that therbligs do not necessarily compose an additive set.

Shumard notes the fact that materials and equipment must be standardized. He discusses job specifications in the relatively large elements in which Taylor originally discussed them.[15]

Stevenson, Jordan, and Harrison point up their adherence to the philosophy of the one best method but fail to indicate to what extent they would break down operations into micromotion therbligs in order to standardize a motion pattern.[16]

Holmes was the first to call for the breakdown of motions into

[12] Dwight V. Merrick, *Time Studies as a Basis for Rate Setting* (New York: Engineering Magazine Co., 1919).

[13] William O. Lichtner, *Time Study and Job Analysis* (New York: The Ronald Press Co., 1921), pp. 21–23.

[14] William H. Schutt, *Time Study Engineering* (New York: McGraw-Hill Book Co., 1943), p. 36.

[15] F. W. Shumard, *A Primer of Time Study* (New York: McGraw-Hill Book Co., 1940), Chapter vi.

[16] Murray Corporation of America and United Automobile Workers, *Production Standards from Time Study Analysis*, p. 46.

muscular reaction breakdowns. He indicates adherence to the primitive mechanical concepts based upon the a priori assumption made by Lowry, Maynard, and Stegemerten in Laws 3 and 4 of their motion economy system.

Carroll expresses impatience with any program of elaborate job improvement in the following words:

> The urge which we all have to find the "one best method" is a very natural, and exceedingly important one. But, it is silly for any one of us to assume that he has sufficient knowledge to find it for any given operation. Even the motion study experts cannot accomplish this result . . . it would appear that they cannot determine upon the best method the first time, but must develop it through many stages.
> It would, therefore, seem fallacious for the time study man to strive for the best method if in so doing, he delays the complete application of measurement and incentive.[17]

It is clear that Carroll accepts the basic philosophy of the one best method, but is anxious to get on with the job.

Thus virtually every practitioner in the field accepts the doctrine of the one best method of doing work. However, unless they are preparing studies for muscular reaction standard data, they are satisfied with comparatively large elemental divisions of the job. In all likelihood, it is this failure to follow the implications of their own logic down into the microscopic standardization of job descriptions in terms of muscular reactions that has lent the standardization of these jobs a limited empirical validity.

Gillespie draws upon no less than the Greek philosopher Plato to contradict the basic assumptions of motion economy. For example, he writes:

> This book denies that the principle of minimum motions is true. This book denies that there is any value whatsoever, outside of a laboratory, in knowing that the time to move a finger through X inches of space is 0.00X minutes. Finally, it denies that a real motion pattern is equivalent to a motion pattern built up of bits of motion.[18]

[17] Phil Carroll, Jr., "Exposing Some Fallacies of Time Study Technique," [1939] *Proceedings of the National Time and Motion Study Clinic* (Chicago: Industrial Management Society [n.d.]), pp. 70–71.

[18] James J. Gillespie, *Dynamic Motion and Time Study* (London: Paul Elek Ltd., 1947), p. 3.

He recommends instead his concept of dynamic motion study. His elaborate listing of the statement of principles of work cycle dynamics shows evidence of interesting psychological insights but leaves us without an objective operational criterion to tell when the operation is ready for measurement.

Abruzzi departs from these qualitative descriptions and merely asks one question: Do the performance times of the workers satisfy some rational operational criteria of statistical stability? He has defined this stability on two levels, local stability and grand stability. Local statistical stability is measured in terms of the variation in production times over a continuous series of items made over a few hours or at most a continuous day. Grand statistical stability is measured in terms of the variation in production rates of a series of small samples taken over an extended time interval from increments considered qualitatively to be taken under essentially the same conditions.

The criteria for local stability consisted of building up Shewhart control charts for the means and ranges from as many as 70 subsamples, consisting of three items each, from a total of 210 continuous readings on a single individual. This was done for individual elements as well as overall cycles.[19]

The demonstration that statistical stability exists is warrant itself for taking the time study.

The extent to which a job can be standardized in terms of micromotion therbligs for performance by everybody will be examined in even more detail in Chapter 14 on standard data.

[19] William Gomberg, "Union Problems in Setting Production Standards," *Proceedings of the third annual Industrial Engineering Institute* at the University of California, February 1951.

CHAPTER 12

Analyzing and Accounting for Allowance in the Raw Data

The most accurate part of time study remains the data on actual observations. Primarily, time study consists of nothing but this original data corrected by a series of approximating correction factors. It is therefore of vital importance that the basic statistic describing the original data be the most accurate available.

Many field engineers have asked, with respect to the work of the I. L. G. W. U. Engineering Department, "Why be so particular about the specific statistic used to describe the original data when the expected margin of error in the correction factors of the original data indicate things that are not in the data itself?" This query brings to mind Charles Darwin's maxim that while a wrong hypothesis can be corrected, a wrong or ignored observation is irretrievable.

The time study is the basic sample that justifies at least the prediction of the future performance of the individual being studied. We have already seen in Part One that this interpretation is the only tenable one if time study is ever to make any pretext to scientific objectivity. Under the circumstances, the least that we can do is summarize the data in such a way as to preserve the validity of the original observations.

The different methods of summarizing and analyzing the basic data in the sample time study itself will, therefore, be evaluated in terms of this criterion.

The first recommendations on the analysis of data were made by Merrick in 1919. The methods that he proposed have been abandoned, but at least one aspect of his recommendations, because it is carried over into modern practice, requires attention. In discussing the data, Merrick observes:

> The striking out of abnormal values, either excessively higher or lower than the average of all the individual times of the same element, is a detail that calls for fine judgment on the part of the time study man. Such variations may be due to an error in reading the watch, or to an abnormal condition of the work that is not likely to recur in the ordinary course of events. While no general rule can be laid down for the elimination of these abnormal items, minimum or maximum isolated items 25 per cent less or 30 per cent greater, respectively, than an adjacent item should usually be rejected.[1]

This reasoning is carried down into modern practice by Lowry, Maynard, and Stegemerten, who write:

> Before taking up the summary of the elapsed times, the time study should be carefully examined for abnormal values. If any are found, they should be indicated so that they can be readily distinguished and excluded from the summary. . . . A value is regarded as abnormal when it is extremely high or low as compared to the majority of the other values for the same element. No definite rule can be established for determining when values are abnormal. . . . Picking out the abnormal values is largely a matter of judgment in which there is little likelihood of error.[2]

This type of reasoning reaches a grand climax in Carroll's recommendations. Carroll reasons from the statement that "no over-all modification made by applying a factor to the whole study, as is so frequently done, can be expected to produce the correct result except in total."[3] Thus in summarizing the data, Carroll omits most of the readings. He recommends assigning

[1] Dwight V. Merrick, *Time Studies as a Basis for Rate Setting* (New York: Engineering Magazine Co., 1919) pp. 12–13.

[2] Stewart M. Lowry, Harold B. Maynard, and G. J. Stegemerten, *Time and Motion Study* (3d ed.; New York: McGraw-Hill Book Co., 1940), pp. 228–229.

[3] Phil Carroll, Jr., *Time Study for Cost Control* (2d ed.; New York: McGraw-Hill Book Co., 1943), p. 85.

to the over-all study a general estimate of performance and then circling those readings which the time study observer believes conform to that estimate of performance.[4] The standard then consists of those readings multiplied by the estimated rating factor. It would be much easier to throw the whole time study away and estimate from judgment what the production rate should be in the first place.

The objection to the omission of out-of-line readings, as recommended by Merrick and by Lowry, Maynard, and Stegemerten, is that there is no scientific justification for the omission of data merely because of relative size. In Chapter 4 we developed a control technique for detecting whether or not a man-machine system was in a state of statistical control during the taking of a time study. However, if the system is not in a state of statistical control, then the physical constant chance cause system behind the phenomena and an assignable cause of variations must be searched for and a new study taken in which this variable is accounted for or eliminated. The elimination of extreme readings without any knowledge of the reason for the extreme reading is merely a distortion of the data and should not be countenanced. There is no reason to rely on unguided judgment in such a situation in view of the new techniques made available by modern statistical practice, such as the control chart. On the other hand, if a reading is to be ignored because of the occurrence of a foreign element for which provision is made in the general allowances, then, of course, the reading may be omitted. However, the rejection of readings merely because they do not look right is a violation of sound practice. If in doubt, the proper thing to do is to take a new time study.

The practice recommended by Carroll is particularly dangerous unless this subject is frankly a matter for negotiation and bargaining. The whole philosophy behind this practice seems to stem from the concept of the search for pattern in time study, expressed by Presgrave as follows:

[4] *Ibid.*, p. 94.

Any recording that is beyond the normal range can not be ignored with safety, in spite of the common practice of discarding extremes. Unusually low timings are especially open to conjecture, since they may provide the key to the whole study. Unusually high recordings are often caused by fumbling, but also may be an indication of conditions that must be corrected or taken into the standard rate.[5]

It is evidently in an effort to search out these assignable causes of variation that Presgrave justifies this practice of selecting by judgment. However, there is a confusion here between the function of time study as a tool of methods improvement and as a measuring instrument for the establishment of a production standard for a job as it exists. Certainly, a study in which these deviations take place should not provide the data for selected times. After the job has been fully investigated, there will be time enough to derive a rational statistic from a collection of data, taken under approximate conditions of statistical control.

What Presgrave is attempting to achieve can be accomplished by means of the control chart technique. Groups of readings that are demonstrated to be out of control should indicate two things to the observer.

1. The job is not ready for a time study.
2. There is an assignable cause of variation that is throwing the system out of control.

It is evidently this latter consideration that interests Presgrave primarily. This, however, does not justify the deduction of a production standard from the data thus restricted. Abruzzi observed that out of 77 factories whose time study practices he surveyed 13 arbitrarily dismissed readings which they subjectively decided were subnormal in determining a production standard.[6]

The general statistics recommended for summarizing data are listed by Morrow as the arithmetic mean, the mode, the selected time, and the composite time.[7] Morrow indicates no

[5] Ralph Presgrave, *The Dynamics of Time Study* (2d ed.; New York: McGraw-Hill Book Co., 1945), p. 185.

[6] Adam Abruzzi, "A Survey of American Time Study Procedures," *Time and Motion Study*, Vol. 2, No. 1 (R. A. Lister & Co., Ltd., January 1953), p. 19.

[7] Robert L. Morrow, *Time Study and Motion Economy* (New York: The Ronald Press Co., 1946), p. 200.

preference for any of the measures. Lowry, Maynard, and Stegemerten recommend adoption of the arithmetic mean.[8] This seems the soundest choice. More is known about the sampling distribution of the arithmetic mean than of any other measure. If industrial time study practice can ever develop more satisfactory methods than it possesses at the present time, in all likelihood it will be as the result of improvements in the correction factor. The arithmetic mean typifies the data of the original distribution and provides the best starting point.

The mode or the value with the greatest frequency is favored by investigators like Piacitelli, as he stated in a lecture series at New York University, 1940. In the Appendix we assume a parent population for time study distributions that would satisfy the Camp-Meidell inequality (p. 280). One of the assumptions behind the use of this inequality, it will be noted, is the coincidence of the mean and the mode. However, we are much more certain of the sampling distributions of the mean than of the mode. Under the circumstances, the mean remains preferable.

The composite time is very widely used in the garment industry. It is equal to one-half the sum of the arithmetic mean and the minimum time. It is used by consulting management organizations servicing the garment industries. There is nothing in sampling theory or, for that matter, any other rational system to justify its use, except perhaps that it has a distinct management appeal. The abuses to which it lends itself are illustrated below.

In warning neophyte engineers on their staff why elemental time study is to be preferred to crude over-all timing, the tables in Figure 11 are given by a leading industrial engineering firm to support elemental times. These tables incidentally shed a great deal of light on the quality of time study technique prevalent in industry.

Through a judicious use of the composite time, a basic standard some 18 per cent lower than would otherwise be the case is derived. Of course, arguments may be advanced that this has been more than compensated by heavy extra-allowances, but why rob Peter to pay Paul?

[8] Lowry, Maynard, and Stegemerten, *op. cit.*, p. 233.

OVER-ALL TIMING METHOD

	Observations														
Complete cycle ..	1	2	3	4	5	6	7	8	9	10	Average	Minimum	Selected minimum	Leveling factor	Adjusted TST
All elements	72	70	70	73	72	70	71	70	72	70	0.71	0.70	0.705	0.70	0.494

ELEMENTAL TIMING METHOD

	Observations														
	1	2	3	4	5	6	7	8	9	10	Average	Minimum	Selected minimum	Leveling factor	Adjusted TST
Element A	4	6	8	4	10	5	8	14	5	6	0.07	0.04	0.055	..	...
Element B	40	20	18	20	30	18	37	19	37	31	0.27	0.18	0.225	..	...
Element C	12	8	6	10	14	6	8	7	13	6	0.09	0.06	0.075	..	...
Element D	10	28	24	22	10	26	10	16	10	14	0.17	0.10	0.135	..	...
Element E	6	8	14	17	8	15	8	14	7	13	0.11	0.06	0.085	..	...
TOTALS											0.71	0.44	0.575	0.70	0.403

Reviewing these tabulations, it is observed that if only total cycle times had been recorded, a minimum of 0.70 min. would have resulted, which is 82 per cent higher than the minimum time of 0.44 min. recorded for the cycle under the second method. While the selected minimum time for the synthetic cycle of elements was 0.575 min., the selected minimum obtained by the over-all timing method would be over 22 per cent higher at 0.705 min. *From this it may be seen that the fundamental principle of time-study work, namely, the timing of the small elements of the cycle separately and then constructing a new synthetic cycle on the basis of selected minimum times, must never be departed from. . . .* (Italics, author's.)

FIGURE 9*

* William Gomberg, "The Relationship between the Unions and Engineers," *Mechanical Engineering*, June 1943, p. 427. Reproduced by permission of the publishers.

We have already eliminated select time, that is, the time which is selected as the result of trained judgment, as a time study technique. This, of course, raises the whole question of whether or not time study can be any better than somebody's presumptuous guess.

All pretext that time study is even approximately scientific, subject to the normal laws of scientific evidence, seems to have been abandoned in this case. Mysterious intuitive abilities become the property of the time study engineer. This sixth sense, this ability to see at a glance what the proper rating of a worker's performance should be, may be developed in some time study technicians, but when challenged, what sort of professional defense is it to say, "I have an intuition," or "My illative sense tells me that this is true." Although it is true that in an uncertain world it becomes necessary at times to proceed on the basis of hunches rather than wait for the decision of able scientific judgment, the odds are that workingmen will prefer their own hunches to those of an outside engineer.

The illative sense belongs strictly to the mystical field of theology. It should not be permitted to cloud the atmosphere of labor relations with unsupported claims to ultimate authority. Unfortunately, this doctrine has been raised to the level of standard professional practice by the statement of the rating committee of the Society for the Advancement of Management.

The worker's opposition to the rating of the intuitive time study expert is seldom based on any carefully thought out methodological approach. The objection is generally based upon an appeal to "common sense" that flows from a firsthand familiarity with the job. This objection naturally has no more scientific claim than the intuitive approach of the engineer, but it has at least the virtue of lack of pretense. No defense is made here for common sense. It is recognized to be as open to question as any other unreliable method of investigation. However, it plays an important role as one of the factors influencing worker motivation. With standards like these, the profession might well abandon any scientific pretense and nail to its masthead the credo

of Anselm, the medieval theologian, "Credo ut Intelligam": I believe, thus I know.

The recent survey of Time Study practices to which we have already referred indicates the following distribution of the uses of the different summarizing statistics.[9]

arithmetic mean	53
median	7
mode	7
selective time	7
typical time	3

Again the reader is warned that the Abruzzi study cannot be interpreted as a good cross-section of American industrial time study practice. Abruzzi is the first to state this. Nevertheless, the information is useful in the absence of any better quantitative data.

Abruzzi has utilized his concepts of local stability and grand stability to develop a sound statistical method for deriving a production standard from data collected in ladies' garment factories under contract with the International Ladies' Garment Workers' Union. It will be recalled that Abruzzi distinguishes between local stability and grand stability. The data which he collects in his examination of the data for local stability is very similar to the array of data which the ordinary time study man takes in the course of a series of continuous observations—with this difference, of course, Abruzzi takes many more readings than the usual time study practitioner in order to satisfy sound sampling criteria.

The collection of data to test for grand stability is quite different. Inasmuch as our primary interest lies in the use of this technique for rate-setting purposes, the techniques that accompany grand stability are of much more importance than those associated with local stability.

Abruzzi analyzed the results of observations on seven different

[9] Adam Abruzzi, *op. cit.*, p. 19.

operations. He divided the working day into the following rational subgroups:

Period	*Duration*	*Characteristic*
1	9:00–10:30 A.M.	morning spurt
2	10:45–11:55 A.M.	morning decline
3	1:00– 2:30 P.M.	afternoon spurt
4	2:45– 4:30 P.M.	afternoon decline

Each of these periods was separated by either a 15 minute rest period or a 1 hour lunch period. Samples of five readings were taken at random times during each of these four periods. The studies covered a five week interval, during which time observations were made on 16 working days. Operation number 1 is typical. It was made up of nine elements and 64 samples of five readings each were taken. These samples on operation 1 included observations on eight different operators. The cycle times alone were analyzed for grand stability. The following table gives Abruzzi's results.

TABLE IV

The Relation Between the Level and the Uniformity of Grand Production rates*

	Operation						
	7	6	5	4	1	3	2
$\bar{X}$ estimate	9.9	12.8	24.4	45.5	59.1	88.1	144.3
$\hat{s}$ estimate	1.7	2.0	2.7	2.9	3.8	5.1	6.0
V estimate	17.2	16.0	11.1	6.4	6.4	5.8	4.2
3 V estimate	51.6	48.0	33.3	19.2	19.2	17.4	12.4†

$\bar{X}$ = arithmetic mean
$\hat{s}$ = standard deviation
V = coefficient of variation

* Adam Abruzzi, *Work Measurement* (New York: Columbia University Press, 1952), p. 109.
† This line added by author.

The significance of these results is that though they comply with a sound statistical formulation, the results are useless for the economic burden which we are compelled to impose upon them. It is true that the longer the cycle time the lower the co-

efficient of variation, but even an operation that lasts as long as 144.3 minutes carries with it a tripled coefficient of variation of 12.4 per cent. Operation 7, which is assigned a standard of 9.9 minutes, carries a tripled coefficient of variation of 51.6 per cent. It will be recalled that the economic criterion which collective bargaining imposes upon rate setting, if rate setting is to be taken out of the area of collective bargaining, is that we are able to demonstrate a maximum tripled coefficient of variation of 2½ per cent at the 99 per cent probability level. This criterion, we may emphasize again, has nothing to do with any scientific consideration. The criterion is imposed by the economic nature of the collective bargaining process. The Abruzzi technique and analysis stands as a major contribution to the intellectual development of time study. His proposed solutions, however, fail to satisfy the required economic criteria demanded by its use for rate-setting purposes.

In the preface to *Work Measurement*, Abruzzi states that according to his viewpoint the procedures of work measurement should be based on the rules of scientific method, not on the bargaining process.

> It seems to me [Abruzzi] that we can no longer tolerate taking empirical action about productivity problems on the basis of the subjective judgments of individuals acting as advocates of vested viewpoints. Instead, we need objective principles and procedures so that the estimates we make and the action we take will be sound in a scientific sense.[10]

This, however, poses a false contradiction. The inherent variability of the scientific finding in this case fails to meet the economic requirements that our subjective sense of equity imposes upon the process. We must, therefore, still bargain over the standard.

I would say that this is virtually the only disagreement between the writer and Abruzzi in his otherwise excellent analysis.

Among the most satisfactory phases of time study practices are the methods that have been developed for special allowances.

[10] Adam Abruzzi, *Work Measurement* (New York: Columbia University Press, 1952), p. viii.

This excludes those for fatigue which we have already treated in our examination of the physiological sources of variation. These special allowances generally provide for (a) special work necessary to the operation, but of a nonrepetitive nature, such as getting new bundles and preparing them for work in a garment factory; or (b) interruptions to the regular work cycle, such as rethreading the machine after a thread break. All other allowances, such as an incentive allowance, machine-cycle waiting time, and delays because of faulty material, remain untreated. The incentive allowance is meaningless, as we have already explained. There is seldom any recognition of the so-called base pay by a worker. Workers think primarily in terms of the earning opportunity afforded by a production standard. This is generally judged by whether or not it permits him to earn his past regular hourly earnings. Machine-cycle waiting time is not considered since we have confined our study to man-paced operations.

There is no way of making provision for faulty materials except on a bargaining basis. If the only justification for time study is the existence of a physical constant chance cause system, and if irregular material is introduced into that system, it becomes impossible to predict what the time necessary to complete an operation will be. When material is defective, it generally means that there was a breakdown in the quality control operation within the plant from which the material came. Thus the distribution of the defects is seldom, if ever, known. Under the circumstances, it is impossible to predict the effect of this material on the time required for an operation.

The allowance for the other two items can be handled in one of two ways. The first is by separating the allowances into variable and constant allowances. The allowance for bundling time is a constant which should be added to the production standard for each bundle of garments independently of the time cycle of sewing. Yet it is not uncommon in the industry to find bundling allowances made in terms of a percentage added to the actual operation allowance. This generally results from the practice described by Morrow wherein the personal, fatigue, and so-called

delay allowances are combined into one gross allowance.[11]

Variable allowances, like rethreading allowances, vary with the time cycle of the operation and are generally computed in terms of a percentage of the operation. The standard methods for determining these allowances, whether it is the all-day production study or the more economic ratio delay technique, are both sound theoretically.

The application of the ratio delay technique to the determination of the variable allowance is the earliest of the sound applications of statistical thinking to a time study problem. Tippett describes its beginnings in the textile industry.

After some thinking and observation, Tippett came to the conclusion that the percentage of looms at any instant actually running in a cotton weaving shed was an indication of loom efficiency during the time interval surrounding that instant. This central idea lies behind the whole ratio delay concept.[12] It was also determined that if snap readings of the state of the looms were taken at random time intervals during a fixed period of time, you would likewise secure an estimate of loom efficiency for that period of time. If the loom was down and the reason the loom was down was recorded with each observation, then at the end of the working period you would have a percentage of loom downtime associated with a specific reason for this downtime.

The extension of this reasoning to allowances for nonrepetitive variable operations becomes apparent at once. You merely observe and record at random intervals whether the operator is doing her regular work. If she isn't, you record what she is doing. At the end of the day you compute the percentage downtime for the particular operation by computation.

$$\frac{S_x}{N} \times 100 = p$$

where S_x = number of observations that worker was doing the x operation instead of the regular operation

N = total number of observations

[11] Morrow, *op. cit.*, p. 169.

[12] L. H. C. Tippett, "Ratio Delay Technique," *Time and Motion Study*, Vol. 2, No. 5, (R. A. Lister & Co., Ltd., May 1953), p. 10.

The number of observations N that are necessary at the 99.7 confidence interval can be computed as follows to ± 2½ per cent:

$$\sigma_p = \sqrt{\frac{p\,(1-p)}{N}}$$

Then unless $\frac{3\sigma_p \times 100\%}{p} < 2\frac{1}{2}$ per cent, we recognize that more observations are necessary. A preliminary estimate of p from empirical sources can be used to estimate our initial N.

Morrow was the first time study practitioner to call the attention of American time study engineers to Tippett's work and make the extension to computing nonrepetitive allowances for man-paced operations.[13] Abruzzi extended Tippett's thinking to the needs of the ladies' garment industry in his program of research.[14] He divides his observations into five different subdivisions during the work day. He then sets up control charts for each of these periods to determine whether or not these percentages are in a state of statistical control. If control exists, then the percentage allowance is final. If statistical control does not exist, then it is a signal to undertake a search for some major cause of variation over which the management must gain control.

[13] Morrow, *op. cit.*, p. 177.
[14] Abruzzi, *Work Measurement*, p. 257.

CHAPTER 13

The "Normal" Worker

In Part One of this study we confined our attention to the power of time study techniques to predict future performance of an individual from past sample time studies. The problem of actual industrial practice is much more complicated than this. In addition to predicting the performance of an individual, there is also the problem of relating that performance to the normal concept.

I have found myself in endless arguments between union business agents and management engineers about whether or not some specified production rate was normal, above normal, or below normal. In almost every case this argument could be reduced to the parent populations each of the persons had in mind. The engineer would be talking about a highly stratified sample of the employed working population, characterized by the factory in which he was employed. The union business agent, on the other hand, would be thinking in terms of the general parent population, made up of all the workers employed in the industry.

Now "normal" has no meaning unless it is defined as some quantitative characteristic of a parent population. Here we can go to two extremes. One extreme is the definition of the parent population to suit Taylor's directions on selecting only first-class individuals for jobs. Naturally, under such circumstances,

normal becomes representative of a highly restricted population. On the other hand, a union may arbitrarily insist that the parent population be assumed to show the wide variations in productivity which Hull's investigations demonstrate (see Fig. 11, p. 193). Under such circumstances, normal production would be frozen at a very low figure.

My belief is that the population concept from which a normal is to be derived must be related to the actual distribution of productivities as they exist, subject to some reasonable improvement in those low sectors of the distribution which lend themselves to an increase as a result of the more superficial techniques of job simplification. Of course, this concept is worded in qualitative rather than quantitative terms. But then, no pretense is made about any scientific validity. As will be apparent in the last chapter, this population can be conceived in terms of collective bargaining.

From the data of Figure 11, Hull concludes that "we shall probably not be in great error if we conclude that among individuals ordinarily regarded as normal in the average vocation, the most gifted will be between three and four times as capable as the poorest."[1]

This range is confirmed by David Wechsler in his 1935 study if we consider most vocations as a combination of motor capacities and perceptual capacities. Wechsler points out that the range of motor functions is in the ratio of 1.65:1. The range of perceptual capacities varies between 2.30:1 and 2.85:1, or a mean of 2.58:1.

It seems rational to assume that human productivity is a complex combination of these various traits listed by Wechsler. For example: suppose we assume that working ability is a vector combination of Wechsler's motor capacities and percepter capacities. A possible interpretation of productivity would be then that the two traits combine as follows:

$$(2.85^2 + 1.65^2)^{1/2} = 3.30$$

[1] Clark L. Hull, *Aptitude Testing* (Yonkers-on-Hudson, N. Y.: World Book Co., 1924), p. 36.

Then the total variation in this new synthetic capacity would be 3.30:1. It would be interesting to design experiments to analyze how the many traits actually do combine to give us the ranges we do find in industry.

In a 1952 revision of his 1935 study,[2] Wechsler gave his own interpretation of the range of human productivity. He wrote in a new chapter, "Range in Productive Operations," that "the range of productivity in industrial operations, between the least and most competent worker on the job, is, on the average, not more than 2.5:1 and generally more nearly 2.0:1."

However, peculiarly enough, he quotes Tiffin in his text to the effect that Tiffin had observed that experienced hosiery loopers ranged in productivity from 2.2 to 7.3 dozen per hour. This is a range from 3.32 to 1. Yet, in the table reproduced below he gives the range of experienced hosiery loopers as limited from 2.57:1. Naturally the predisposition of an industrial psychologist would lead him to key a norm to a selection technique that would confine the working force to the upper range of the natural distribution. Trade unionists prefer to measure the range of those workers whom they find in the industry. It will be noted that in every case the number of observations from which Wechsler's ratios are derived are considerably less than 500.

The ranges in capacity listed by Hull and derived from Wechsler are extremely conservative because of the small sample size from which these values are taken. Sound statistical procedures require a minimum sample size of 500 in order to justify the assumption that the observed sample *extreme values* are the same as the population *extreme values*. The sample extremes will be, of course, at least as wide as the population extremes, but there is a good chance that the population extremes will be even wider.

The concept of the "normal" worker has been at the bottom of much of the conflict between management and labor.

[2] David Wechsler, *Range of Human Capacities* (2d ed.; Baltimore: Williams and Wilkins Co., 1952), p. 69.

TABLE V
Range in Productive Operations

Operation	Unit or Criterion	Subjects	Mean	S.D.	Extremes	Ratio
Typing	Number of words per minute	616 adults	53.4	9.57	85–30	2.83:1
Manipulating automatic lathe	Average output per day	120 adult females	71.8	6.85	104–51	2.04:1
Card punching	Aver. no. of cards punched per hour	113 adults on day shift	232.2	29.8	340–160	2.10:1
Card punching	Aver. no. of cards punched per hour	121 adults on night shift	198.5	31.1	275–115	3.00:1
Machine sewing	Units per hour (5 day average)	101 adult females	72.4	14.3	112–43	2.55:1
Filament mounting (elec. bulbs)	Aver. no. per hour	100 adults	104.9	13.5	132–75	1.73:1
Assembling radio tubes	Number tubes done per hour	65 adults	99.5	16.9	132–61	2.00:1
Checking grocery orders	Time in seconds	46 adults	365.0	—	575–225	2.53:1
Hosiery looping	Dozen per hour	99 experienced loopers	4.0*	—	7.2–2.8	2.57:1
Checking, posting and listing	Per cent of aver.	34 adult females	102.1	20.7	150–65	2.30:1

* Median.

Surveys of American industrial time study practice disclose that virtually all companies use some subjective method of revising or correcting the data collated for setting production standards. Barnes, in a study of 89 companies, demonstrated that all but one of them used some method of correcting data.[3] Abruzzi has shown a very similar result in his survey of American time study practices. These practices vary widely, but almost all of their users claim an accuracy for their techniques of anywhere from plus or minus five to ten per cent. The difference

[3] H. A. Lynch, "Rating of Time Studies—A Progress Report," *Modern Management*, Vol. lx, No. 5, 1949, pp. 16–19.

in concept between validity and precision is never even defined, much less distinguished. Both concepts are buried in the amorphous term, accuracy. The rating process, if it is to be meaningful in even an empirical sense, depends upon a completely operational definition of the concept of validity and precision. Validity is the term associated with the legitimacy of the level of work itself, defined as the standard. Precision defines the limits within which the standard is reproducible at a specific probability level.

It is interesting to analyze existing rating methods and the rationale underlying them with these concepts of validity and precision clearly in mind.

The professional engineering societies have recognized that this rating concept must be standardized if the occupation of the time study technician is ever to secure professional recognition.

The Society for the Advancement of Management some years ago set up a rating committee to study this whole problem. There is still some confusion about what the committee was to search for and what the committee finally did find. We shall have occasion somewhat later to look into the detailed report of the committee with special attention to the more controversial aspects of the study.

The definition of the rating process laid down by the National Committee on Rating discloses its completely subjective character. They say: "Rating is the process during which an observer evaluates the performance of the operator in terms of his own mental conceptions of the proper or normal performance of that job."[4] In other words the validity criterion is completely subjective by definition.

The Northern New Jersey Chapter of the Society for the Advancement of Management had set up its own rating committee somewhat earlier. They attempted to tie down this validity concept by the following definition of normal.

[4] "Rating of Time Studies." Issued by the Rating Committee of the Society for the Advancement of Management, 1952.

The normal represents an unstimulated rate of production (or effort) that should be expected from a fully qualified operator. The fully qualified operator, being considered as one who has been working on a job long enough to know it thoroughly and who possesses normal intelligence with enough education to perform satisfactorily the work assigned to him.[5]

Mundel gives a definition of normal which is equally subjective. He states: "The standard time for a job will be 130/100 of the amount of time that will be necessary to accomplish a unit of work using a given method, under given conditions by a worker possessing sufficient skill to do the job properly, as physically fit for the job, after adjustment to it, as the average person who can be expected to be put on the job, and working at the maximum pace that can be maintained on such a job day after day, without harmful physical effects."[6]

However, he protects himself by recognizing the tautological nature of the definition and the equitable concepts involved. He states that the union is entitled to bargain for a statement defining adequate performance.[7]

Originally there were two schools of thought representing the principal trends of thinking about the rating process among industrial engineers. The so-called leveling approach has been advanced by Lowry, Maynard, and Stegemerten. The concept of effort rating has been advanced primarily by Presgrave and Carroll.

Subsequently, new schools of thought have been introduced into the rating field; there are the synthetic leveling proposed by Morrow and the objective rating technique proposed by Mundel.

We shall turn first to an examination of the relative merits of the two original methods of leveling and effort rating.

Now, if normality is defined as the arithmetic mean of the parent population which is to be accepted as exhibiting the distribution of acceptable production rates, it becomes somewhat

[5] Northern New Jersey Chapter, The Society for the Advancement of Management, A Progress Report of the Committee on the Rating of Time Studies, reprinted from *Industrial Engineer*, April 1943, p. 1.

[6] Marvin E. Mundel, *Motion and Time Study* (New York: Prentice-Hall, Inc., 1950), p. 322.

[7] *Ibid.*, p. 315.

difficult to determine the merits of the conflict between the Lowry-Maynard-Stegemerten school of leveling and the Presgrave-Carroll school of effort rating. This conflict takes place on two levels: (a) the factors which go into the rating process; and (b) the allowable range within which the variation of productivity falls. The first is largely academic; the second is exceedingly important inasmuch as it determines the population of acceptable workers in a factory.

Presgrave uses Wechsler's estimate of the range of human motor capacities of 1:2.23.[8] An examination of the origin of the table from which this figure was computed indicates that it is made up of capacities such as flexion of wrists, tapping, simple reaction times, *et al.*[9] In other words, the perceptual function of the work at hand has been completely eliminated. Presgrave is within his rights in using this range since he restricts this range to speed of movement only. However, his attempt to restrict rating to measuring the speed of movement only is open to question. The range which he finally accepts is 1:2.25.[10]

Lowry, Maynard, and Stegemerten, on the other hand, indicate a range of productivity of 2.76:1, that is, from −50 per cent to +138 per cent. Although the normal should be defined as the midpoint of the range, the three investigators protect themselves by recommending the discharge of those with skills below average.[11] As the result of a process of selection, the normal would no doubt fall in the upper part of the actual distribution, specifically at 2.00, instead of at the midpoint.

Lowry, Maynard, and Stegemerten's allocation of these allowances is shown in Figure 10. Skill is defined as proficiency at following a given method.[12] Effort is defined as the will to work.[13] Consistency is self-explanatory. The authors warn that

[8] Ralph Presgrave, *The Dynamics of Time Study* (2d ed.; New York: McGraw-Hill Book Co., 1945), p. 96.
[9] Wechsler, *op. cit.*, p. 149.
[10] Presgrave, *op. cit.*, p. 119.
[11] Stewart M. Lowry, Harold B. Maynard, and G. J. Stegemerten, *Time and Motion Study* (3d ed.; New York: McGraw-Hill Book Co., 1940), p. 209.
[12] *Ibid.*, p. 207.
[13] *Ibid.*, p. 216.

inconsistency should be a signal to investigate the operation.[14] Conditions are physical conditions of work. The authors give as an example the fact that unusual cold may make the fingers of a skilled operator somewhat stiffer than usual.[15]

Although Lowry, Maynard, and Stegemerten do not indicate how these allocations were derived, they claim that they were worked up from comparison studies of about 175 men over a period of years before 1927. Without the original figures, the origin of these allocations cannot be investigated. However, the allocations do not seem to possess too strong a rational base.

Presgrave attacks this method of leveling primarily because skill is listed as a leveling element. Skill is assigned 37 per cent

Skill			Effort		
+0.15 +0.13	A1 A2	Superskill	+0.13 +0.12	A1 A2	Excessive
+0.11 +0.08	B1 B2	Excellent	+0.10 +0.08	B1 B2	Excellent
+0.06 +0.03	C1 C2	Good	+0.05 +0.02	C1 C2	Good
0.00	D	Average	0.00	D	Average
−0.05 −0.10	E1 E2	Fair	−0.04 −0.08	E1 E2	Fair
−0.16 −0.22	F1 F2	Poor	−0.12 −0.17	F1 F2	Poor
Conditions			Consistency		
+0.06	A	Ideal	+0.04	A	Perfect
+0.04	B	Excellent	+0.03	B	Excellent
+0.02	C	Good	+0.01	C	Good
0.00	D	Average	0.00	D	Average
−0.03	E	Fair	−0.02	E	Fair
−0.07	F	Poor	−0.04	F	Poor

FIGURE 10*—THE WESTINGHOUSE SYSTEM OF LEVELING

* *Ibid.*, Fig. 106, p. 233. Reproduced by permission of the publisher.

[14] *Ibid.*, p. 242.

[15] *Ibid.*, p. 241.

of the total of 88 per cent in the Westinghouse scheme (see Fig. 10). Presgrave objects that skill is a matter of method, to be taken care of by motion analysis. He states:

> Motion analysis and correction are not matters of leveling factors or of rating, but must be achieved by selection, by elimination and by adjustment.[16]

> To rate for skill, to attempt to measure it and express it by a number, is a fault that all time study men fall into in some degree.[17]

Presgrave is quite right when he points out that it is difficult to reduce skill to a number. But will motion analysis solve the problem of relating skills by standardizing motion patterns? We have already seen from the findings of the industrial psychologists that this is highly doubtful. Elimination might be acceptable if job opportunities were abundant. Until they are, it is unacceptable as a remedy. Adjustment is but another word for what Lowry, Maynard, and Stegemerten are attempting to do, however inadequately. Thus Presgrave may be right in asserting that Lowry, Maynard, and Stegemerten have not treated skill correctly, but if he is right, it is for the wrong reason.

Presgrave is quite correct when he argues that one operator performs at a higher rate of productivity than another for one or both of two reasons:

1. He performs identical motions with greater rapidity.
2. He performs the operation in a different manner.[18]

But taking the latter problem out of the field of time study merely transfers it to another field where its basic insolubility will still bedevil time study techniques.

We may conclude that relative skill must be accounted for in time study practice. Until it is accounted for, it is just as well to confess that it constitutes one more nail in the coffin of the pretensions which time study makes to a scientific technique.

Consistency and uniformity of performance are a basic part

[16] Presgrave, *op. cit.*, p. 68.
[17] *Ibid.*, pp. 68–69.
[18] *Ibid.*, p. 71.

of skill. It seems somewhat unnecessary to divide them from skill as separate factors.

It is unwise to place a ceiling on allowances for conditions. If they are bad, they should be corrected. If they cannot be corrected, then it is unnecessary to set up any range within which a correction factor must fall. Suppose they are so bad that 20 per cent is needed!

It might be argued that if the leveling system does not seem to stand up on the basis of a priori considerations, at least it does demonstrate some empirical validity. Strauss and Cohen have attempted to test the validity of the technique.[19] The two items of leveling which were tested were the effort and skill elements.

A highly repetitive operation of the Mine Safety Appliance Corporation, Pittsburgh, Pennsylvania, was selected for the study. The operation studied was completely man-paced. The differences in performance of motion paths among the operators were quite subtle.

The operation involved the folding of a gauge sheet 18 inches square to a size of approximately four inches square. This was accomplished in six foldings. Twenty-one completely trained female employees performed this job daily. All of them were rated by three observers while their performance was filmed by a synchronized movie camera which exposed a fixed number of frames per second. The observers consisted of Mr. H. B. Maynard and two members of his staff, who rated by means of pooled judgments. There is no record of the deviations among the engineers. The following table indicates the accuracy of the results.[20] If the leveling technique is valid, then the figures derived by multiplying the actual frames taken by the leveling factor should all equal one another.

The figures are quite eloquent. They range from a deviation of +25 per cent to −44 per cent.

[19] Leonard Strauss and Leonard Cohen, "Time Study and the Fundamental Nature of Manual Skill," *Journal of Consulting Psychology*, May, 1946.

[20] *Ibid.*, unnumbered.

TABLE VI*

Time Study of Operators' Performance in a Folding Job

Operator No.	Leveling Factor	Actual No. of Frames Taken	(L.F. × A.F.T.) Leveled Time
17	123	48	59.04
1	111	81	89.91
10	108	83	89.64
20	105	93	97.65
16	100	99	99.00
7	86	145	124.70

* Reproduced by permission of the publisher.

A micromotion analysis *indicated differences* in method. These differences, however, were not in the sequences of operations but in the subtle combinations of therbligs. Thus Presgrave seems mistaken in thinking that these differences can be eliminated by a training program, no matter how thorough. On the other hand, if the leveling technique is no more accurate in the hands of its founders when assisted by two assistants whom we can presume are indoctrinated with the same ideas, some other answer will have to be found if time study is to survive or at least gain acceptance.

Subsequent conversations with Mr. Maynard at a joint meeting of the American Society of Mechanical Engineers and the Society for the Advancement of Management on time study, held on April 27, 1946, disclosed that Mr. Maynard agrees that his leveling method fails to account for subtle differences in motion patterns used.

Thus the status of the Westinghouse leveling technique may be summed up as follows:

The range of performance of 1:2.76 for the workers employed in a vocation is not unreasonable, but it is tight when compared with what Hull and Wechsler disclose. The attempt, however, to define the normal at 2.00 in the ratio of 1:2.76 does seem somewhat unreasonable. The technique if carried to its logical conclusion would place speed demons at 138 per cent with the normal defined at 38 per cent less than the speed demon. This would confine employment to a very highly stratified

group in the population. Such an approach would be unacceptable to any trade union.

In reviewing Presgrave's concept of effort rating, the principal objection to the procedure is that it seems to place a premium upon just plain speed-up. I had the opportunity to witness an application of the method by one of its leading exponents. The logic was sound, but the empirical results were disastrous. The Management Engineering Department of the International Ladies' Garment Workers' Union has on file a film of three operators all performing the same operation of sewing a dart in a brassiere. Operator A is rated at 84 per cent, Operator B at 100 per cent, and Operator C at 190 per cent. These figures reflect their comparative productivity as measured by a wink counter in the camera field. Operator C is using a slightly different method from Operators A and B. All attempts to teach A and B the same motion sequence used by Operator C are of no avail. In all likelihood this preference has a firm foundation in the individual differences basic to their respective make-ups. When an observer was asked to rate Operator C, he rated her at 110 per cent on the basis of the relative pace she was maintaining. The ratings for Operators A and B were very close to the true relative productivity figure. It is quite easy to see what the result of the application of this rating procedure would mean. Employees studied would of necessity be those whose motion pattern was the most productive. Inasmuch as it is speed alone that is being rated, and the overwhelming number of operators are those whose motion patterns are not perfect, the logical application of this method would leave most operators without any bonus under an incentive plan. If, on the other hand, we follow Carroll's directions not to await the one best method but merely to speed-rate existing methods,[21] then every time a rate, as the result of some dexterous redesign of an operator's motion pattern, exceeds the pre-assigned range met by

[21] Phil Carroll, Jr., "Exposing Some Fallacies of Time Study Technique," [1939]*Proceedings of the National Time and Motion Study Clinic* (Chicago: Industrial Management Society [n. d.], p. 70.

the speed rating procedure, the plant engineer must demand the right to reset the rate. The only sound basis upon which Presgrave's effort rating becomes a useful concept is the assumption that all people can be trained to use the identical motion pattern down to the last muscular reaction. Presgrave's dispute with Segur on this score indicates his disbelief of this assumption. Under the circumstances the usefulness of effort rating must be confined to the very simplest type of repetitive operations, and even there it remains suspect.[22]

Presgrave is well aware of these defects. He lists the following limitations to effort rating:

1. The difficulty of rating above or below a certain range of speed.
2. The difficulty of rating certain types of operations.
3. The problems of method.[23]

The first limitation is a limitation of the rater's ability as the extreme of either range is reached. The second involves jobs, such as clipping garments sewn in a chain, which are purely a function of a worker's dexterity. Presgrave assures us that these jobs are becoming fewer and fewer as the process of mechanization and de-skilling continues. This is a highly tenuous assumption.[24]

The first limitations of method are those very considerations which we have already listed. This leaves a very small area in which effort rating can be useful.

Barnes has performed some interesting work in an effort to determine to what extent a rater can be trained to estimate pure speed accurately. Although his results show that there was some improvement in the ability of time study men to rate walking accurately, he warns: "No claim is made that the result of a

[22] See Segur's letters to Presgrave in *Motion Time Analysis Bulletin*, A. B. Segur & Co., November 2, 18, 29, 30, 1944; May 1945; and September 1945.

[23] Presgrave, *op. cit.*, p. 168.

[24] Subsequent conversations with Mr. Presgrave at the same meeting at which Mr. Maynard appeared confirmed this information. Mr. Presgrave agreed at that time, that effort rating falls down where wide differences in productivity are traceable to minute differences in motion patterns used by different workers.

walking experiment is a true index of the overall ability of a man to set time standards."[25]

Under the circumstances, it is somewhat difficult to assess the distinctive value of the effort-rating technique. Certainly under the circumstances, factors other than speed must, in the last analysis, be resorted to.

Morrow has proposed a system of synthetic leveling. Basically what happens is that an operation is broken down into elements. A table is kept of often used elements for which a standard time has been developed. Actual observations are then taken on these elements. It is understood, of course, that this is done exclusively for the man-paced elements. The machine time and machine-paced elements are excluded. The leveling factor is then computed for the entire study, including those elements for which there are no standard times. The computation is as follows:

L equals the leveling factor;
A is the observed time for the known element;
S is the standard time for the known element.

$$L = \frac{S}{A}$$

The usefulness of this method depends upon two assumptions: The fundamental standard times for these known elements are valid. We shall examine the validity of such an assumption in our chapter on standard data. The other assumption is that there is a uniform relationship in the speed of the different elements to the speed of the over-all cycle. This latter assumption has been proven completely false by Schwab.[26]

Mundel's objective rating sets no limits on the possible vari-

[25] Ralph M. Barnes, "Work Measurement—An Experiment in Rating Operator Performances," [*1944*] *Proceedings of the National Time and Motion Study Clinic* (Chicago: Industrial Management Society, 1945), p. 14.

[26] P. W. Schwab, "An Investigation to Determine the Proportionality of Element and Therblig Times," unpublished Master of Science thesis, Purdue University, 1948.

ation within the working force. He defines objective rating as follows:

> Objective rating consists of the following steps:
> 1. The rating of observed pace against an objective pace-standard, which is the same for all jobs. In this rating, no attention whatsoever is paid to job difficulty and its effect on possible pace; hence, a single pace-standard may be used instead of a multiplicity of mental concepts.
>
> 2. The use of a secondary adjustment consisting of a percentage increment, added after the application of the numerical appraisal from step 1 has been used to adjust the original observed data. This percentage increment is to be taken from experimentally determined tables of the effect of various observable factors that control the exertion required at a given pace.[27]

The first part of the operation is relatively simple. It is straight speed rating. The secondary adjustment in step two resembles the technique of job evaluation. It is essentially a device to serve the psychological needs of the analyst. It is a method whereby he projects himself into the position of the worker and asks himself: How much ought I add for the relative difficulty of maintaining this job at this pace. Mundel himself confesses that the presently available data for secondary adjustments leaves much to be desired.

Kalman Lifson of Purdue University has done some pertinent work in analyzing the effectiveness of this method of rating.[28] Lifson's study was unique in that it listed the following purposes among others:

> To compare the worker's judgments with the time study men's pace ratings. To determine the effects upon the pace ratings which may result from actual performance of the jobs by the time study men.

Four very elementary man-paced jobs were chosen. They were typical small assembly jobs of the semiskilled variety like twisting screws into threaded holes. Five workers were then chosen who were Purdue University students. All of them had

[27] Barnes and Mundel, *op. cit.*, p. 322.

[28] Kalman A. Lifson, "Errors in Time Study Judgments of Industrial Work Pace" (adapted from, "A Psychological Approach to Pace Setting," a doctoral dissertation, Purdue University), American Psychological Association, Inc., August 1951.

worked previously as factory workers and were accustomed to a worker's method of assessing pace. None of them had any time study experience or training. Each of the five workers performed each of the four jobs at fixed paces and a movie was shot of each of these workers at their task. The workers were given an opportunity to evaluate each pace on the film. Six time study men made pace ratings of the performances on the films, and one month later, after three of the time study men had had an opportunity to perform the jobs, these same three were given an opportunity to rate these films again. The six time study men all had had extensive experience in industry. Two of these men were members of the Purdue University faculty in industrial engineering, where Mundel developed the objective method of time study rating.

The time study observers used the objective rating method to which they were accustomed and in which they were trained. Lifson developed a unique method of getting the worker's evaluation of these jobs. The worker's judgments were quantified by having them translate their judgments in money terms; for example, a worker was told that by working at the 100 per cent pace for job number two he could earn $1.00 per hour. He was then given another job at one of the fixed paces and asked: "If you received $1.00 per hour for performing job number two at the specified speed, what do you think you ought to be earning now?" Each worker was taken through every job in this manner and was thus able to rate each job by alternating the standard with a regular operation.

The worker's judgments were then cross checked by making another job standard, which he had equated to the 100 per cent pace for job number two, and, using this new standard, taking him through every job in the same way.

Finally, a third choice of a standard job was made, and the worker compared each job with this standard. In this way a worker was able to compare each job against a standard at least three times. Three separate judgments of each job were made by the workers.

Lifson was able to draw three very significant conclusions from his data:

1. Workers' judgments on equating the jobs differ from the pace ratings. The workers believed there was as much as a 12 cent per hour differential between two normal paces as established by time study men.
2. Some workers can judge more reliably than time study men can rate. The standard error of the judgments of the two most consistent workers was ¾ the size of the standard error of the best time study men.
3. Doing the job changes the time study men's pace ratings. The concepts of normal on jobs changed.[29]

We shall have occasion to refer to Lifson's very significant study again. Meanwhile, these three conclusions would tend to support the rationale behind objective rating as the most useful of the empirical techniques yet available provided we do not restrict ourselves to Mundel's correction factors. The workers' judgments of the relative burden that different jobs impose upon a fixed pace are likely to be at least as reliable as the conclusion of the observing, expert time study man.

First hand familiarity with the job sharply influences the mental concept of normal envisaged by the time study observer.

All other rating systems found in industry seem to be derivatives of the basic systems outlined above.

Stevenson, Jordan, and Harrison emphasize that "the rating technique evaluates rate of performance, not the operator's ability as a workman."[30] This indicates adherence to the effort-rating school of thought as well as some careful attention to the feelings of the workers involved.

Norris and Elliott reproduce Lowry, Maynard, and Stegemerten's laws of motion economy and their whole approach is leveling.[31]

Schutt recommends what might be termed the circular method of rating. First, the general approach is that of Lowry, Maynard, and Stegemerten with a slightly different allocation of

[29] *Ibid.*, pp. 45–46.

[30] Murray Corporation of America and United Automobile Workers, *Production Standards from Time Study Analysis*, p. 58.

[31] Norris and Elliott, Inc., *Engineering Manual* (Unpublished), Sect. 2.2.3.

percentages to various factors. The relationship between the top rating and the average is still in the ratio of 1:1.38. His distinctive contribution arises in the application of the technique. First he takes the average time for each element. Then by a series of snap-back readings he picks what he considers the 100 per cent times. These 100 per cent times are divided by the average times to give the efficiency ratings.[32] Why go through this rigamarole? Mundel has pointed out that it would make more sense to take the readings wanted in the first place.[33] Yet Schutt observes that "when you can back up your judgment with time values obtained through the repetitive check, no one can dispute your decision."[34]

Shumard's concept of the brisk maintainable pace seems to be along the lines of speed rating developed by Presgrave and Carroll.[35]

Barnes originally recommended the kind of leveling advocated by the Westinghouse group (Lowry, Maynard, and Stegemerten).[36] However, his recent work indicates a shift of interest to pure speed rating.

Holmes is dissatisfied with the Westinghouse method and rejects it in favor of pure speed rating. He states that "with a speed standard and a knowledge of what can and should be done, and what motions are necessary and unnecessary, they [considerations of skill and consistency] are a useless expenditure of time."[37]

It is at once apparent that nothing has been developed in industrial time study practice that can be considered an objective

[32] William H. Schutt, *Time Study Engineering* (New York: McGraw-Hill Book Co., 1943), pp. 65–74.

[33] Marvin E. Mundel, "An Analysis of Time Study Rating Systems and Suggestions for a Simplified Systematic System," *Industrial Engineer,* Vol. IV, 1944, p. 23.

[34] Schutt, *op. cit.,* p. 70.

[35] F. W. Shumard, *A Primer of Time Study* (New York: McGraw-Hill Book Co., 1940), p. 67.

[36] Ralph M. Barnes, *Motion and Time Study* (2d ed.; New York: John Wiley & Sons, 1940), p. 273.

[37] Walter G. Holmes, *Applied Time and Motion Study* (New York: The Ronald Press Co., 1938), p. 200.

measure of normality or an objective method for comparing operator performance with any normal standard.

The confusion in the field of rating is pointed up by the recent report of the S.A.M. on the rating of time studies. The one part of the report in which the writer can concur with the sponsors of the project is in their assessment of the rationale behind most rating systems today. Their conclusion can be summed up as follows: None of us seem to know quite what we are doing or why, but somehow the results we have been getting correlate and are satisfactory. We readily subscribe to the first part of this statement. The authors are very careful to point out that the section of the report on pages 11–12 which castigate existing rationales or lack of rationales possesses a somewhat different status than the remainder of the report. They write: "Obviously many of the readers of this report, including members of the rating committee, will not agree with many of the statements. It is important to remember that the only purpose of this portion of the report is to stimulate thinking on the part of the reader."

What follows is an exhaustive and devastating critique of the claims made for the different methods in use. The confusion between methods changes and skill and the confusion and lumping together of rating allowances with allowances for fatigue are all examined along very much the same lines we have pursued. They summarize the situation pretty well in two well chosen paragraphs:

> Hundreds and hundreds of different systems are capable of producing satisfactory empirical results, as is clearly indicated in the survey results. Through experience, the users of even a badly designed system can "warp" it sufficiently to make it work satisfactorily. Often practice does not coincide with expressed theory.
>
> Since a rating factor is a ratio, hundreds of rating scales are possible. Since fatigue allowances are almost invariably arbitrary, almost unlimited variation in practice is possible, and lastly, since allowances practices affect the way rating of short-term performances is done, an almost infinite number of combinations of rating scales and allowances is possible.[38]

After such a set of conclusions, the authors complain that it is

[38] "Rating of Time Studies," issued by the Rating Committee of the Society for the Advancement of Management, 1952, p. 12.

most unfortunate that no strong society of industrial engineers can compel standardization in the midst of this chaos. Inasmuch as this chaos merely reflects the lack of knowledge in the field, we are at a loss to understand what standardization can accomplish at this juncture. All that it would mean is that the engineers would protect themselves against public criticism by surrounding the technique with an officially sanctioned code of gobbledygook. Officially sanctioned codes of standards at this time can only be useful to those who need the protection of an organization "ethical code" as they attempt to ram their technique down the throats of workers without any embarrassing questions asked.

To what extent does the detailed examination by the S. A. M. Rating Committee of present production standard setting techniques justify discussing such a code at the present time?

Source	Vocation	Criterion	Ratio of poorest to best worker
Loveday and Munro	Heel trimming (shoes)	No. pairs per day	1:1.4
Elton	Loom operation (silk)	Per cent of time loom kept in operation	1:1.5
Pollock	Hosiery maters	Hourly piecework earning	1:1.9
Wyatt	Loom operation (fancy cotton)	Earnings	1:2
Loveday and Munro	Bottom scoring (shoes)	No. pairs per day	1:2
Pollock	Knitting-machine operators	Pounds of women's hose per hour	1:2.2
Scott and Clothier	Office boys	Weekly salary	1:2.3
Hertzberg and Thiel ...	Elementary teachers	Ratings of superiors	1:2.5
Farmer	Polishing spoons	Time per 36 spoons	1:5.1

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FIGURE 11*—SHOWING THE RATIO OF THE LEAST EFFICIENT TO THE MOST EFFICIENT INDIVIDUAL ACTUALLY ENGAGED IN A VARIETY OF GAINFUL OCCUPATIONS

* Clark L. Hull, *Aptitude Testing* (Yonkers-on-Hudson, N.Y.: World Book Co., 1928), Table 7, p. 35.

CHAPTER 14

The S. A. M. Rating Study

The unabridged report[1] of the S. A. M. committee on the rating of time studies has been available for some time. The cover labels the study "a report on a research program undertaken to study existing practices in industry, to establish a concept of a fair day's work, and to develop films as a medium for training in rating."

The foreword explains that "the society did not contemplate officially sponsoring a standard of performance for manual work. However, among other things, it was interested in learning whether or not there was substantial agreement among engineers as to what constituted a 'fair day's work.' The project shows that agreement was excellent."

Later a letter and an enclosure over the signature of Vincent D. Flynn, research director of the Society for the Advancement of Management, dated September 15, 1953, lists a number of questions and answers on the S. A. M. performance rating films. Question 9 asks, "Of what use are the S. A. M. performance rating films?" One answer among others states, "to provide bench marks that will hasten the solution of disputes arising out of the setting of production standards . . . to provide a source of standard data or motion time values, or to check existing values."

[1] "Rating of Time Studies," issued by the Rating Committee of The Society for the Advancement of Management, 1952.

What Mr. Flynn has in mind is disclosed in the cover letter, which begins, "Since the issuing . . . of the S. A. M. performance rating films . . . as a means of standardizing throughout industry the concept of a fair day's work."

To all intents and purposes the disclaimer that the bench marks are unofficial is meaningless. Operationally these films have become the official Society code. The Society has been doing all in its power to gain universal acceptance of these standards.

This conclusion follows logically from one of the stated purposes of the investigation: "The Rating of Time Studies Project is mainly concerned with establishing a nationwide concept of proper performance, and providing means by which Time Study Engineers can improve their performance rating."[2]

We are primarily interested in examining this study to determine whether or not the statement in the foreword that the study gives conclusive evidence that agreement among industrial engineers over what constitutes a fair day's work is excellent is justified by the evidence presented in the report. We refer the reader again to the positive aspects of the study listed in the last chapter. We have already indicated that the most valuable part of the report is the comment on the meaningless rationales underpinning most time study rating systems. The feelings of offended professionals are spared by the careful delineation of this part of the report as completely unofficial; it does not even bear the official imprimature of the rating committee itself, much less the Society, though it is by far the most thoroughly thought out part of the report.

We are now ready to examine the data collated by the authors of the report, its validity, its treatment and the conclusion drawn from the data. We first begin with an analysis of the actual collating and validity of the original data.

Films were made of 24 different, simple, industrial, clerical and laboratory operations. Some of these films were made by the investigators. Others were acquired from industry sources.

[2] *Ibid.*, p. 3.

We are assured that all of the changes in pace on these films are man-made, that is, they are not contrived by varying the speed of the projector. The different schemes were arranged in random order so that the tendency to rate comparatively would be minimized. Absolute rating was sought.

After these films were prepared, selected, and edited, the individual scenes were rated by 1,800 men from some 200 companies. The collection of this data was accomplished in three ways. Three companies conducted the survey under controlled conditions on their own premises.

The engineer in charge of the project made special visits to 12 other plants. Special rating meetings were held of S. A. M. chapters in a number of other cities. In each case engineers were asked to rate, using the rating scale to which they were accustomed. In many cases the papers of some raters were discarded because the survey committee did not consider them qualified time study men. It is significant that only 60 per cent of the raters at these meetings were selected for inclusion in the study. Among the criteria used for discarding raters were less than three months experience or a statement by the group supervisor that the individual was not fully trained in time study. All other groups and individuals were included. This would seem to mean that about 40 per cent of the time study men faced by workers are incompetent even by S. A. M. standards.

The raw data is thus made up of a collection of single estimates. We can summarize the raw data of the main investigation as follows. They are made up of single-shot readings of a single worker on a specific operation at each pace.

We now proceed to evaluate the nature of this data for the purposes stated by the authors of the study.

If what was wanted were bench marks of manual performance of a number of common industrial operations, would not the following procedure have made much more sense and yielded more meaningful data? Pick for your jobs simple assembly tasks or other jobs common to a great many factories,

circulate the movies of the jobs among the same engineers or their supervisors, and then ask them two questions: Is this job being performed in your factory? If the answer is yes, then: what production are you getting per hour over an extended period as revealed by your actual production records? You would thus be getting real objective data.

Instead, as we see, each engineer was asked to offer his prejudice about how much work the management was entitled to receive from the workers.

The most important limitation of the collation of the data, however, is that each rater rated a single job at a single speed once and only once for the principal study.

We have already referred to the Kalman Lifson study. His conclusion will be recalled that three of the highly experienced time study men changed their rating substantially after they had had an opportunity actually to perform the operation. If these three men had been included in the S. A. M. study data, it would be concluded that the original rating that these men had performed was a consistent rating which could be recalled consistently on presentation of the same scene.[3] Lifson went on to observe that for some inexplicable reason some workers are consistently rated higher than others, even when all perform the same job at the same pace. He states, "A standard time set on the worker who was rated the highest would have been 15 per cent longer than the one set on the worker who was rated lowest."[4] The design of the experiment by the S. A. M. committee calls for the same single worker to be seen in the same job at the same pace.

Kinsler has given further evidence of the limitations built into the data. He is a statistician attached to the time study department of the Oscar Mayer packing house in Madison, Wisconsin. Much of his time has been devoted to an investigation of company problems in rating and developing indices of effi-

[3] Kalman Lifson, "A Psychological Approach to Rate Setting," unpublished doctoral dissertation, Purdue University.

[4] *Ibid.*, pp. 44–45.

ciency for individual raters. He asks the question, "Why have any index at all? If all we want to know is how do I rate today, we can rate a film and then draw a chart comparing our ratings with the known film ratings."[5]

This is exactly what the designers of the S. A. M. investigation have done. Kinsler also wants to know how a rater's ratings on the same jobs vary from day to day. Kinsler found much variation in his single company among raters from day to day. He was led to develop a completely new set of indices for evaluating raters. We shall examine the nature and importance of Kinsler's indices when we examine the methods of analyzing and treating the raw data. It is adequate for our purposes at this time merely to point out that Kinsler found this problem in an excellently directed standards department enjoying consistent homogeneous supervision over work in a fairly restricted field. Kinsler found this source of variation a problem even when the company's raters rated these self-same S. A. M. films.[6]

We can well imagine how much greater a problem this inconsistency would be among the heterogeneous group of observers from which the S. A. M. data was collected. The authors were to some extent aware of this problem. They do point out in their analysis of the so-called anchoring effect that when the same set of scenes were shown in two different sequences there was as much as a 21 per cent shift in the group concepts of the normal performance. They claim that the variation from this source was eliminated by the randomization of the scene sequence.

However, the single shot readings eliminate any possibility of getting any picture of the day to day variances of the individual observers. It is somehow implied though not stated explicitly that the positive variances of one group of subjects will counterbalance the negative variance of another set of subjects.

[5] Philip J. Kinsler, Research Statistician, General Industrial Engineering Dept., Oscar Mayer & Co., "Measuring Rating Ability," unpublished paper, 1953.
[6] *Ibid.*

This is the equivalent of taking a mean deviation without changing the signs around the arithmetic mean and then concluding that the variance of a distribution is unimportant because, eureka! the mean deviation is zero.

We now turn to the methods used by the investigators to analyze and treat the data.

The authors give an excellent account of their method of treating the data in the report. They make use of two basic statistics to reflect the measures of variability, the systematic error and the absolute error. These measures are adapted from the work of Ralph M. Barnes.

1. The systematic error is the average percentage amount by which the ratings made by each person deviate from the calculated ratings—the algebraic mean of the errors.

2. The absolute error is the average percentage amount by which the ratings of each person differ from the calculated ratings when all deviations are considered plus quantities.

Both of these concepts are spelled out in the illustrative hypothetical examples taken from the original report.[7]

Sample Tabulations: (Hypothetical Data)

Scene	*Individuals* 1	2	3	4	5	*Average Rating*	*Cycle Time*	*Normal Time*	*Calculated Rating*
Z-1	125	120	115	125	130	123.0	.0292	.0359	126.2
Z-2	100	105	110	105	100	104.0	.0340	.0354	108.3
Z-3	150	145	140	150	155	148.0	.0243	.0360	151.6
Z-4	155	150	145	150	160	152.0	.0238	.0362	154.7
Z-5	70	75	80	85	82	78.4	.0519	.0407	71.0
								.1842 ÷ 5	

Average Normal Time = .03684

Steps in Computing Systematic Error and Absolute Error:

1. Add the individual ratings of the 5 men in group for Scene Z-1 and divide by the number of men = Average (Mean) Rating.
2. Multiply Average Rating by Cycle Time (known in advance) = Normal Time.
3. Add the Normal Times and divide by Number (5) = Average Normal Time.

(Note: The first three steps were done as part of the procedure of computing common denominator times.)

[7] "Rating of Time Studies," *op. cit.*

4. Divide Average Normal Time by each Cycle Time (same Cycle Time value as step 2) to obtain Calculated Rating. This of course is the proper or correct rating—the one that will produce the Average Normal Time.
5. Next, using the Calculated Ratings as base figure compute each individual's percentagewise deviation to one decimal place on each of his ratings. For example: Man "1" rated Scene Z-1 at 125. Since Calculated ("correct") value is 126.2 his deviation is $\frac{-1.2}{126.2} \times 100 = -1.0\%$. Each deviation must be identified as either a plus or minus quantity.
6. To compute an individual's *systematic error* on an operation, total his plus deviations and minus deviations separately, subtract smaller quantity from larger and retain sign of larger quantity, then divide by number of scenes. For example: Man "1"—

Scene	*Rating*	*Calculated Rating*	*Deviation*	
			Plus	Minus
Z-1	125	126.2		$\frac{1.2}{126.2} = 1.0\%$
Z-2	100	108.3		$\frac{8.3}{108.3} = 7.7\%$
Z-3	150	151.6		$\frac{1.6}{151.6} = 1.1\%$
Z-4	155	154.7	$\frac{0.3}{154.7} = 0.2\%$	
Z-5	70	71.0		$\frac{1.0}{71.0} = 1.4\%$
		Totals	+0.2	−11.2

Steps in Computing Systematic Error and Absolute Error (Continued)
Subtract 0.2 from $-11.2 = -11.0 \div 5 = -2.2$ = Systematic Error of Man "1" on operation Z.

7. To compute Absolute Error add plus and minus deviation totals together considering both as plus quantities and divide by number of scenes. For example: $0.2 + 11.2 = 11.4 \div 5 = 2.3$ = Absolute Error of Man "1" on operation Z.

Overall Systematic and Absolute Errors are also computed as an average of all the operations the individual rated.[8]

The investigators then faced the problem of taking all this data and converting it to a common denominator. The basis for this common denominator was the amount of work expected

[8] "Rating of Time Studies," *op. cit.*, pp. 39–40.

by the management under the wage system in use at the end of an eight hour day.

Let us assume that the average normalized time for an operation under the rating system of the company in question is .0428. The authors of the study then demonstrate the conversion in the following language: Company number 22 uses an allowance factor composed of 4 per cent personal, 10 per cent fatigue, and 1 per cent general delay. These allowances were designed to be additive; therefore the factor is 1.00 + .04 + .10 or 1.15. Company number 22 states that 105 per cent is the expected attainment of the average of qualified incentive operators on an eight hour day basis on their rating scale. The common denominator time then is:

$$\frac{.0428 \times 1.15}{1.05} = .0469$$

These common denominator times were then used to compute the so-called composite time value: It is defined as follows by the authors: "The time value obtained for each of the 24 operations when the common denominator times of all companies in the survey are averaged. The composite time value is a weighted average with weighting being done on basis of the number of men in the company group."[9]

We now turn to an evaluation of the method of analysis of the data.

The basic statistics, systematic error and absolute error, are completely ineffective and misleading measures of deviations from central tendency. They are virtually useless as indices of the prediction of future performance. Dr. Ralph M. Barnes[10] indicates that he has abandoned these measures for all current research and is working closely with competent mathematical statisticians to develop sound parameters from the field of mathematical statistics.

The authors state that there are alternative methods of statistical treatment but complain that the limited statistical knowl-

[9] *Ibid.*, p. 6.

[10] According to a conversation with the author at the University of California at Los Angeles during the summer of 1953.

edge of the audience for whom the report is intended makes a more complex treatment of the data impractical. This is the first time, perhaps, that we have seen professional limited competence cited as a reason for using misleading measures to arrive at emotionally satisfying conclusions, completely unwarranted by the data.

Kinsler gives an interesting example of the completely misleading nature of the use of the systematic error and absolute error. He lists his company's observer A's ratings for the card dealing sequence in reel one. He then lists the readings for observer B and shows that the indices for both subjects are the same.[11]

True Rating	*Ratings by Observer A*	*Ratings by Observer B*
115	110	110
145	125	145
103	95	84
134	115	126
90	90	70
Systematic error for both	—10.40	
Mean deviation for both	7.28	
Absolute error for both	10.40	

We now picture the rating patterns of subjects A and B on Figure 12.

According to the indices both A and B have the same rating pattern. The graph shows them to be altogether different. The S.A.M. study solves this problem by ignoring it. Yet the S.A.M. is aware of Kinsler's work and describes his technique with obvious approval in the condensed version of *A Fair Day's Work*.[12] However, the S.A.M. study seems to overlook the implications of the application of the Kinsler technique to their own study. Kinsler's technique shows how misleading these single shot readings can be, with which the S.A.M. attempts to prove widespread agreement among industrial engineers. In addition, it demonstrates the uselessness of the concept of the systematic error and the absolute error.

[11] Philip J. Kinsler, *op. cit.*, pp. 3–4.

[12] A report of the Research Division of The Society for the Advancement of Management, 1954, pp. 70–73.

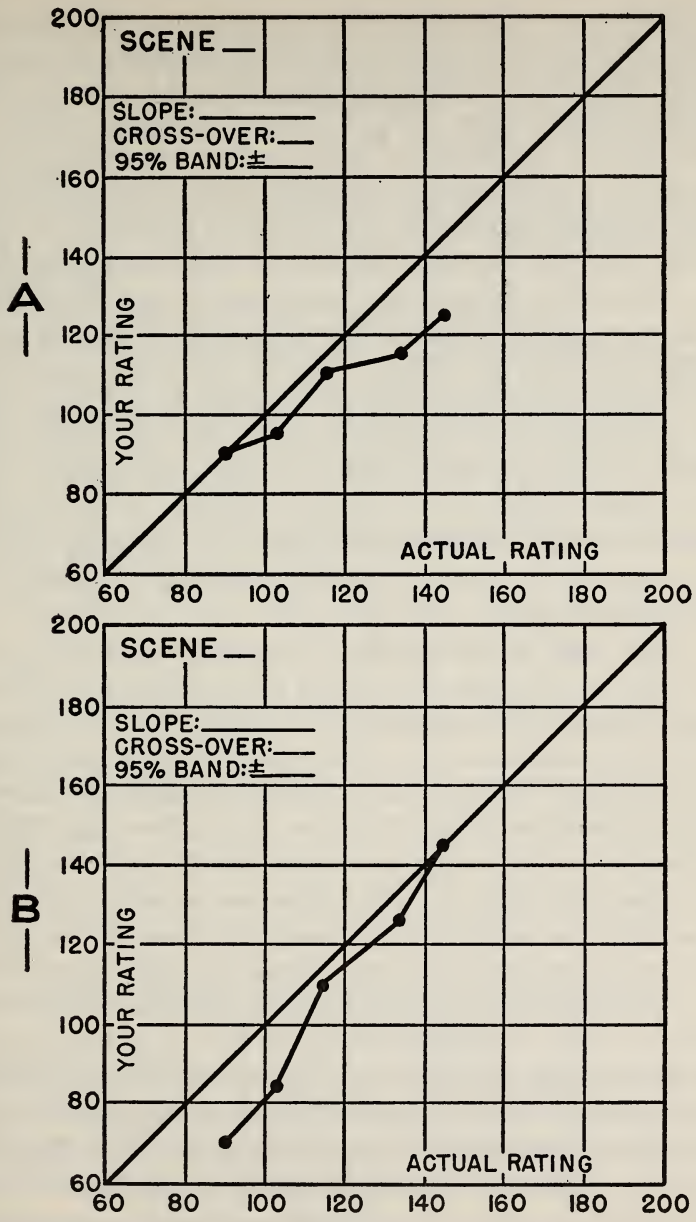

A
SCENE
SLOPE:
CROSS-OVER:
95% BAND:±
YOUR RATING
ACTUAL RATING
200
180
160
140
120
100
80
60
60 80 100 120 140 160 180 200
B
SCENE
SLOPE:
CROSS-OVER:
95% BAND:±
YOUR RATING
ACTUAL RATING
200
180
160
140
120
100
80
60
60 80 100 120 140 160 180 200

FIGURE 12

Similarly the common denominator time is a doubly subjective concept dependent upon two unrelated subjective guesses by the time study men. We have already referred to guess number one, the intuitive rating of the data itself. Now still another intuitive guess is introduced. A group percentage of the average earnings for a wide variety of operations for a large number of people is applied to a single specific item of manufacture. What is more the group average is applied to single time study readings individually. Anyone who has operated with wage incentive payment plans is aware of the wide range of earning opportunities offered by the different items of work. This shows itself in the workers distinction between "good work" and "bad work." This amorphous overall average, which is only meaningful when applied to a group of items made by a group of workers is now given specific application to a single reading on an item of manufacture with which the time study man is only vaguely familiar. The concept of the common denominator time is ingenious but hardly objective data.

We now turn to the criterion of accuracy proposed by the study. The investigators are careful to specify that consistency is meaningless without accuracy. They write: "To be consistent involves only determining the relative difference in performance between scenes." Since we are establishing standards, accuracy is also required. An analysis of the procedure shows the criterion of accuracy to be an ingeniously contrived "average normal time," the result of the pooled subjective prejudices of all the engineers in the company. The composite time of the over-all criterion of accuracy for all the companies is similarly contrived.

An examination of the basic illustrative example demonstrates that the so-called calculated ratings are the result of the process of successive approximations. This process could be indefinitely repeated until you would show virtually no deviations for any observer from the measure of central tendency. It is an ingenious device to achieve a self-demonstrating hypothesis. It is the height of folly to set up this artificially contrived measure as the basic standard of accuracy. The final result of all this heroic

arithmetic must eventually be to prove that the variation of sample means around the population mean is less than the variations of the individual measures around the population mean. You hardly need an investigation to prove this self-evident hypothesis.

Naturally, this technique leads to the very results that the investigators are seeking. The results are built into the experimental design.

The investigators are finally able to report that:

1. The average percentagewise deviation on all 24 operations of the company composite time values varied between 6.0 per cent and 6.6 per cent on two different scene arrangements with ranges that varied from scene sequence one of 5.0–6.8 to scene sequence two of 5.0–7.6.[13]

2. 599 individual raters showed systematic deviation ranging from –20.80 to 19.04 per cent.[14]

3. Similarly the same 599 individual rates showed absolute errors varying from a low of 6.40 per cent to 21.70 per cent.

4. The A. E. again for the same group was 10.57 per cent. For the same group on sequence one it was 10.79 per cent. On sequence two it was 10.35 per cent.[15]

In addition to all of the limitations we have already noted to the significance of the systematic and absolute deviation we can add still another. Even if we assume that the concept of the absolute deviation is meaningful, the per cent deviation of factory X from the composite time for all the factories equals

$$\frac{\text{over-all composite time} - \text{factory X composite time}}{\text{over-all composite time}}$$

This badly understates the variation of rating judgment of the observers because the composite time equals observed time or rating increment. Even for this limited method, therefore, the true measure of variation or rating efficiency would be

[13] "Rating of Time Studies," *op. cit.*, p. 53.
[14] *Ibid.*, p. 82.
[15] *Ibid.*, p. 84.

$$\frac{\text{over-all composite time} - \text{factory X composite time}}{\text{possible rating increment only}} \text{ or } \frac{\text{composite observed time} - \text{rating time}}{}$$

We can illustrate by an example. Suppose it were possible to get composite ratings instead of composite time. Suppose further that the over-all composite rating was 125 per cent. Suppose that factory X yielded a composite rating of 122 per cent. Is the true rating efficiency $\frac{25 - 22}{25} \times 100$ per cent, or is the true rating efficiency $\frac{125 - 122}{125} \times 100$ per cent? The choice of denominator would make considerable difference in our opinion of these ratings. The S.A.M. method can be manipulated to show as high a rating efficiency as we please.

What is true about the comparison of factory composite time values is even more true about absolute errors for individual ratings. That is why it is so amazing to find that the investigators finally reject their own results because they feel that they overstate the variance among the individual raters. They are led to reject their own quantitative conclusion that the average absolute error of 10.56 per cent is representative of the rating error found in industrial time study work.[16] The writer would agree that the variance is greater.

The results of the study, therefore, can be summarized as follows. Much as the committee attempted to devise measures to show that their project results indicate that agreement is excellent among engineers over what is a fair day's work, they did not succeed. They, therefore, dismiss their findings and declare such is the case anyway.

Equally startling corollary conclusions follow, topped by the climactic proof that "we must conclude that concept of proper performance is largely independent of wages paid." In other words, engineers want what they want no matter what wages are.

As limited as the original data is, it is nevertheless entitled to

[16] *Ibid.*, p. 60.

decent statistical treatment that will give us limited empirical results. We suggest the following design. It is virtually certain that this treatment will show much greater variability than the methods used by the S.A.M. investigation, which they wind up rejecting after all their effort. It may not prove what they want, but it will give a picture of the data and its implications. The design of such an investigation follows. It is based upon the concepts of analysis of variance.

Let A be operation dealing cards
Let B be operation transport marbles
Let C be operation toss blocks
etc., for all operations

Let ${}^{k}a_{ij_k}$ designate operation of dealing cards in either rating units or time units in company k at i speed by observer j_k.

We can now designate the array $^{1}/A$

S	${}^{k}a_{11_k}$	.	.	.	.	${}^{k}a_{1n_k}$
P	.					.
E	.					.
E	.					.
D	${}^{k}a_{m1_k}$	.	.	.	.	${}^{k}a_{mn_k}$

The array pictures the operation dealing cards at m different speeds as rated by n different observers in company k.

Note that for ${}^{k}a_{ij_k}$ and ${}^{p}a_{ij_k}$ that observer j_k is different from j_p but that ${}^{k}a_{ij_k}$ designates the same speed as ${}^{p}a_{ij_p}$ for the card dealing operation.

We now proceed to some definitions. ${}^{k}a_{11_k}$ . . . ${}^{k}a_{1n_k}$ designates a distribution of ratings by n different observers of the operation dealing cards at a single speed i. We shall designate distribution of this type as follows:

${}^{k}A_1$ Dealing cards in company $_k$ scene 1 all observers
${}^{k}B_1$ Transport marble in company $_k$ scene 1 all observers
${}^{k}A_2$ Dealing cards in company $_k$ scene 2 all observers
${}^{k}B_2$ Transport marble in company $_k$ scene 2 all observers
etc.

Compute arithmetic means and confidence intervals and the variances for

$^{k}A_1; {}^{k}A_2 \ldots\ldots, {}^{k}A_m$
$^{k}B_1; {}^{k}B_2 \ldots\ldots, {}^{k}B_m$
each operation in each factory for k, p, q. . . . .

This will give the means and variances of the ratings for all the observers at each specific speed for each specific operation. This information will give us the percentage limits inside of which any company observer in a specific firm can be expected to deviate from the company mean for a specific operation at a specific speed at a chosen level of probability. It tells us nothing, however, about whether or not the same raters will rate the same scenes differently at different times. Unfortunately, this limitation has been built into the fundamental data by "single shot readings."

Now let us take the distribution

a_{1j_k}
a_{2j_k} Designate this array A_{j_k}.
a_{3j_k}
a_{mj_k}

This is the distribution of times reflecting ratings of m different scenes for $_j$ observer in Company $_k$ operation a. We determine the arithmetic mean and variance and confidence interval for each such operation in each company. This should give us the predictable consistency and the safe range for different speeds for this one observer. Again our interpretation is limited by the single shot readings to the doubtful assumption that perfect consistency can be expected at the same speed.

We now determine whether any deduction from $^{k}/A$ is meaningful. We compare the variances of the means of the distributions $^{k}A_1$, $^{k}A_2 \ldots\ldots {}^{k}A_{mk}$ (same speed, different observer) with the variances of the means of distribution A_{1k}, A_{2k}. . . . . , A_{mk} (same observers, different scenes) with the total variance.

If this data is homogeneous, then and only then is a mean and confidence interval justified for this data. Naturally, all dimensions are in time. This would complete the study for a single operation within each company.

We are now ready to compare the same operations at different speeds between different companies.

$$\begin{matrix} {}^{k}\bar{A}_1 & {}^{k}\bar{A}_2 & \ldots\ldots & {}^{k}\bar{A}_m \\ {}^{p}\bar{A}_1 & {}^{p}\bar{A}_2 & \ldots\ldots & {}^{p}\bar{A}_m \\ {}^{q}\bar{A}_1 & {}^{q}\bar{A}_2 & \ldots\ldots & {}^{q}\bar{A}_m \end{matrix}$$ All units are in time.

If it is determined that these means and their variances constitute a homogeneous group from the use of the analysis of variance technique, we are then justified in abstracting a common mean for this array with the appropriate confidence interval. This would give us an overall measure of central tendency for a single operation.

Now we wish to compare different operations within the same company for the same observers. Here we are restricted to $\frac{\text{variances}}{\text{arithmetic means}}$ because means and variances by themselves become meaningless. They are not comparable. The new index of the $\frac{\text{variance}}{\text{means}}$ makes them comparable.

$$\frac{\sigma^2}{\bar{x}}{}^{k}/A$$
$$\frac{\sigma^2}{\bar{x}}{}^{k}/B$$
$$\frac{\sigma^2}{\bar{x}}{}^{k}/C$$
$$\frac{\sigma^2}{\bar{x}}{}^{k}/Z$$

This column of $\frac{\text{variance}}{\text{means}}$ describes the array comparing different operations in the same company. Again we must determine whether or not they are a homogeneous array.

We can do this by extending this concept to describe the array of $\frac{\text{variance}}{\text{means}}$ between different operations in different companies as follows:

$$\frac{\sigma^2}{\bar{x}}{}^{k}/A \quad \frac{\sigma^2}{\bar{x}}{}^{p}/A \cdots\cdots \frac{\sigma^2}{\bar{x}}{}^{q}/A$$

$$\frac{\sigma^2}{\bar{x}}{}^{k}/B \quad \frac{\sigma^2}{\bar{x}}{}^{p}/B \cdots\cdots \frac{\sigma^2}{\bar{x}}{}^{q}/B$$

$$\vdots \qquad \vdots \qquad \vdots$$

$$\frac{\sigma^2}{\bar{x}}/Z \quad \frac{\sigma^2}{\bar{x}}{}^{p}/Z \cdots\cdots \frac{\sigma^2}{\bar{x}}{}^{q}/Z$$

Again we test for homogeneity of data before combining the data and abstracting a mean and confidence interval. Before undertaking such a heroic task of arithmetic one may very well ask: What will we have when we are finished? The answer is just a few measures of central tendency which are not necessarily related to any criterion of accuracy. The investigators can now decide whether the effort is worth while. The analysis they present is meaningless.

The optimistic conclusion about the consistency of American industrial engineers is not shared by English industrial engineers about their own practice.

A similar study, *The Consistency of Stop Watch Time Study Practitioners,* was made by Messrs. Rodgers and Hammersley.[17] They measured the inconsistency between observers by taking the standard deviations of percentage variations. They estimated an over-all 21 per cent standard deviation as the error "which one would expect from a typical observer drawn from the aggregate of all time study men of various creeds and organizations."[18]

This means that if the average final figure of a group of particular observers is X, about 2/3 of the observers in this group

[17] Winston Rodgers and J. M. Hammersley, "The Consistency of Stop Watch Time Study Practitioners," *Time and Motion Study,* Vol. 3, No. 9, September 1954.

[18] *Ibid.,* p. 21.

may be expected to produce final figures not differing from X by more than plus or minus 21 per cent of X. About 95 per cent of the observers would fall within the limits of plus or minus 42 per cent of X. This is purely a measure of consistency. It has nothing to do with any level of absolute accuracy. It can be interpreted as measuring the *inconsistency* within such an aggregate.

It might be added that the English investigators compensated in their study for a factor called Variation in Company Policy. This factor is the equivalent of the device used by the S. A. M. investigators to reduce all their data to a common incentive expectancy level.

The rigorous standards we have chosen to measure the report are dictated by the claims made on behalf of the report by the Society. It has been the advertising program of the Society in selling these films that has led us to the harsh demands of rigor and perfection that we have a right to expect from an "official" Society code.

It is questionable whether or not the worth-while purposes of the Society are advanced by an energetic sales promotion program in which advertising techniques are permitted to play hob with the very excellent scholarly reputation the Society has hitherto enjoyed. Raising money through the sales of these films to finance research purposes is a worthy objective.

However, the most important purpose the Society has hitherto served has been that of a scholar's forum where material presented before the audience was completely open to the criticism of the scientific community. Doctrines presented there either survived or fell depending upon the ability of their proponents to establish and defend the scientific validity of the rationale behind their recommendations.

A review of the Society's literature fails to disclose the publication of any nonofficial material critical of the methods or findings of the investigation. Discussions of the film have assumed the appearance of sales promotion efforts rather than serious attempts to evaluate what has been done. As a result the

American Federationist carried a special editorial denouncing the imposition upon workers of such a unilateral study. Walter Reuther, president of the C. I. O., in a special memo to his officers, denounced the study and warned union officers against accepting any of these bench marks as arbitration guides in disputes over production standards.

Competent observers like Adam Abruzzi have isolated the process of rating from the rest of time study. In discussing rating specifications he writes:

> Since these specifications are not scientific but ultimately based on value judgments, it is likewise clear that each party should have the right to develop its own value judgments and set of specifications if it so chooses. The result is that the specification-setting aspect of work measurement is actually reduced to a bargaining problem which, it will be shown, can profitably be considered within the framework of the theory of games.[19]

He then goes on to recommend strategy for management. He says:

> In fact, the explicit admission that rating and specification-setting are a game rather than scientific processes would in itself be of advantage to management; it would prevent successful labor challenges simply on the basis that the process is not scientific.
> The only situation in which it is advantageous to management to retain the claim that its ratings and specifications are scientific occurs when it knows but labor does not know that they are not scientific. The claim of scientific validity may then be sufficiently persuasive so that the rating results and standards are accepted without challenge. In that case, consistent biases in applying the process would in the long run favor management. However, it should be noted that this strategy is an extremely risky one since it becomes ineffective as soon as labor becomes aware of the true nature of the process.[20]

Presgrave sums up the situation when he finally remarks that "time study still remains as much an art as a science."[21] In view of the complications presented by rating operator performance in addition to the theoretical difficulties already discussed, in

[19] Adam Abruzzi, "A Realistic Model for Rating Procedures," a paper—*Proceedings of 17th Annual Time and Motion Study Clinic*, 1953.

[20] *Ibid.*, pp. 12–13.

[21] Ralph Presgrave, *The Dynamics of Time Study* (2d ed., New York: McGraw-Hill Book Co., 1945), p. 194.

predicting the future performance of the same operator from past sample time studies, it is doubtful that there can be such a thing as scientific rating in time study.

The one bright hope that is left is standard data. As Presgrave puts it:

> All these bars to the accurate evaluation of operators tend to disappear when full and intelligent application is made of the "standard-data" technique. Here we make use of the fact, already explained, that effort rating exists in default for a complete statistical picture.[22]

Let us look at standard data in the following chapter.

[22] *Ibid.*, p. 176.

CHAPTER 15

Standard Data

The development of standard data from which all industrial motion patterns could be synthesized was proposed by Taylor at the very inception of the time study technique. Taylor stated:

> No system of time study can be looked upon as a success unless it enables the time observer, after a reasonable amount of study, to predict with accuracy how long it should take a good man to do almost any job in the particular trade, or branch of a trade, to which the time student has been devoting himself.
>
> It is true . . . that all of the work done in a given trade can be divided into a comparatively small number of elements or units and with proper implements and methods it is comparatively easy for a skilled observer to determine the time required by a good man to do any one of these elementary units.[1]

As we have already stated in our chapter on the standardization and improvement of basic methods (Chapter 11), the theory of standard data presupposes the validity of the atomistic view of psychology. It further presupposes that the elements of which a job is composed make up an additive set, that is, that the time values assigned the elements are independent of the position in which they appear. We shall examine these assumptions exhaustively later.

Basically, standard-data systems may be divided into two

[1] Frederick Winslow Taylor, *Shop Management* (New York: Harper & Bros., 1911).

categories, the macroscopic and the microscopic. These two classifications are not in watertight compartments, but represent the polar extremes of two distinct tendencies.

The macroscopic school generally formulates its data in terms of sizable job elements that reappear in many operations. Outstanding exponents of this school are Carroll and Lowry, Maynard, and Stegemerten—the latter two prior to their espousal of Methods Time Measurement.

The microscopic school formulates its data in terms of minute muscular reactions or therbligs. Leaders of this school of thought are A. B. Segur, R. M. Barnes, W. G. Holmes, the Work Factor group made up of J. H. Quick, W. J. Shea, and R. E. Koehler,[2] the M.T.M. group,[3] and the basic motion group associated with Presgrave's company, J. D. Wood and Gordon Limited.

We first turn to an analysis of the macroscopic systems of standard data.

The key to Carroll's system is the comparison sheet.[4] Time study observers enter their elemental readings for the same element alongside one another. Thus, instead of having one estimated reading to serve as a guide, many estimated readings serve as a guide. However, the basic data is no stronger than the rating method that was used to secure it in the first place. The primary advantage the method has over the single study is that instead of one observer's shaping the time for the element (with the consequent possibilities of prejudice), many observers' readings are used, and thus pooled judgment is trusted to compensate for the basic weakness of the underlying rating method.

Similarly Lowry, Maynard, and Stegemerten enter their leveled elemental readings on a master table of detail time studies for comparison purposes.[5] Here we have experimental

[2] J. H. Quick, W. J. Shea, and R. E. Koehler, "Motion-Time Standards," *Factory Management and Maintenance*, Vol. CIII, No. 5, May 1945, p. 157.

[3] H. Maynard, G. Stegemerten, and J. Schwab, *Methods-Time Measurement* (New York: McGraw-Hill Book Co., Inc., 1948).

[4] Phil Carroll, Jr., *Timestudy for Cost Control* (2d ed.; New York: McGraw-Hill Book Co., 1943), p. 112.

[5] Stewart M. Lowry, Harold B. Maynard, and G. J. Stegemerten, *Time and Motion Study* (3d ed.; New York: McGraw-Hill Book Co., 1940), p. 305.

evidence of how far off the pooled judgment of three observers under the Westinghouse leveling method can be. All that is needed is to recall the work of Cohen and Strauss to determine how far from accurate the readings may be with standard data.[6]

Nevertheless, Carroll is right in a sense when he states that standard data gives more consistent results than individual studies. This will be true even if the basic data at the foundation of the plan are questionable. The reason for this, it may very well be suspected, is that the workers feel that standard data at least reduces to writing an implied bargain between them and the management. The standard data virtually constitutes a contract which is not likely to be changed daily with every new time study. The workers who have a pretty good idea of how much the management expects them to earn, adjust their motivating drive to earn this wage. The engineer's curve is kept in balance, the workers are fairly secure in their earning opportunities, and the engineer's predictions come out correctly even if it is for the wrong reasons. If no other case could be made out for standard data, such practical success would be sufficient justification for this use. However, this hardly constitutes an answer to the fundamental question that time study sets for itself. This cycle of reactions (chiefly psychological) probably accounts for the successful application of a wide variety of plans, all of which give different standards.

Barth, Taylor's mathematical wizard, who did most of the practical work in establishing the now-abandoned Merrick method for time study analysis, suspected that some such motivating force was in operation. In replying to the Gilbreths' attack on stop-watch time study, he stated:

> It is hardly conceivable that two time study men, however well equipped by training and experience and with physical means, would arrive at exactly the same time allowance for any job each might in turn be independently assigned to study. And still, the time allowance of either would undoubtedly be fully satisfactory for use in establishing a fair con-

[6] Leonard Strauss and Leonard Cohen, "Time Study and the Fundamental Nature of Manual Skill," *Journal of Consulting Psychology*, May 1946.

tract between the worker and the management, though the two would not be identical.[7]

This basic information was kept clearly in mind when a system of standard data was worked out for the determination of piece rates in the New York dress industry. The New York dress branch of the International Ladies' Garment Workers' Union utilizes a macroscopic system of standard data which has been in successful use for 16 years. It remains substantially the same as when developed by a group of industrial engineers in 1935. The secret of its success is that it is viewed as a completely pragmatic method. It introduces a greater degree of consistency into the settlement of piece rates than would be available without it. A runaway rate or a tight rate is not a cause for alarm; it is taken for granted that they will occur because of the very limitations of the technique. No compunction is felt about changing a rate initially set by the "Unit System" as it is called. The system is a guide to a pragmatic process. It is not a scriptural code for an authoritarian superstition.

Any attempt to achieve more than this with standard data opens itself to a much more rigid set of criteria. A macroscopic system of standard data which would stand undefeated in all its findings must meet the following set of challenges. Its advocate must prove:

1. That the view of human behavior of the atomistic psychologists is the only valid one or, more specifically, that the Gestaltists are wrong.
2. That the arbitrary divisions into which time study elements are divided are independent of one another.
3. That these elements constitute an additive set.
4. That the basic data from which these elements are derived has been treated by sound statistical methods.

An examination of the basic data of practically all macroscopic standard data systems discloses that subjective leveling

[7] *Symposium on Stopwatch Time Study*, p. 108.

has been used to derive the standards. This eliminates criterion four at once.

The other three criteria have been examined by Abruzzi, and a review of his work in this field is most rewarding. Abruzzi took the elemental break downs which had been made by engineers of the union staff and the management group in two different garment factories and subjected them to the Wilks mutivariate test for independence.[8] The tests disclosed that not only were the elements in these operations not independent, they were actually tied together by a complicated network of relationships. These relationships varied with the number and the magnitude of the elements involved. They were different from one operator to another. They were different within the distributions of the same operator.

This should not have been surprising to any student of elementary psychology who had begun the first chapter of his course on the nature of individual differences.

Some experimentation was done to determine whether or not independence could not be attained by combining elements into element groups. Some success was achieved but again the number of elements required to form an independent group varied from plant to plant and from operator to operator.

Until more satisfactory evidence is presented that the macroscopic standard data systems rest upon sounder foundations than those which we have disclosed here, their use will be restricted by competent practitioners to completely pragmatic empirical purposes.

The microscopic standard data systems have become much more fashionable of late than their less elegant relatives in the macroscopic class. MacLatchie, in a burst of enthusiasm, stated before the Industrial Management Society in 1946, ". . . I am certain the day is not far off when industry will discard the

[8] Adam Abruzzi, "Developing Standard Data for Predictive Purposes," *Journal of Industrial Engineering*, Official Publication of the American Institute of Industrial Engineers, Inc., Columbus, Ohio, Vol. III, No. 3, November 1952, pp. 15, 22–24.

stopwatch, will abolish leveling, and will establish time study rates directly from fundamental motion-time standards."[9]

The microscopic approach to time standards has now practically pre-empted for itself the term "predetermined time standards." The current enthusiasm for predetermined time standards has been developing among management for some time. A factory[10] survey disclosed that 132 users of these predetermined methods were very happy with them. However, it is significant that in a great many cases the respondent for the company was the head of the time study engineering standards department. It is safe to say that these installations would not have been undertaken without his positive recommendations. It would indeed be foolhardy for the respondent to jeopardize his own position in the company with a confession that the system, for whose installation at substantial expense he shared responsibility, was not working out. It is nevertheless interesting to note that although 97 per cent of the respondents agreed that the predetermined time standards were accurate enough for the purposes to which they were put, 80 per cent of them continued to check their results with a stopwatch. Eighty-seven per cent of them attributed better shop methods to the use of the system, though it is difficult to see what the time standard system did for methods which would not have been achieved through an ordinary methods improvement program as Anne Shaw has already pointed out.[11] Respondents were about evenly divided between those who felt that predetermined motion times reduced grievances and those who felt that they made no difference or made matters worse. The important thing, however, is that this is adequate evidence that both a practical and theoretical examination of the technique is due once again.

[9] Robert MacLatchie, "Wage Incentives from Motion Time Standards," [1945] *Proceedings of the National Time and Motion Study Clinic* (Chicago: Industrial Management Society, 1946), p. 59.

[10] *Factory Management and Maintenance,* Vol. III, No. 9, September 1953, pp. 134–139.

[11] Anne Shaw, *Quarterly Bulletin No. 2,* Organization Ltd., June 1953.

Opportunities to investigate and examine microscopic systems of data are not too encouraging. Disclosure of the basic information ranges from Segur's complete secrecy to relatively limited information about final results and fundamental motion time assignments. Little is given about the fundamental data and its treatment. The advocates of Methods Time Measurement have made a limited disclosure by telling us something of their methods and giving us a complete table of their fundamental motion times.[12] Similar material has been published by the Work Factor Company. Lynch discloses like information for Basic Motion Time Study.[13]

Most of the data published are an attempt to show that standards set by the methods advocated result in production standards that are met under a wage incentive payment plan.[14] I have seen no evidence of any attempt to publish the methods and detailed data from which the basic muscular reaction elemental time assignments are derived. Some years ago I stated:

> What is the reaction of a union engineer to be then when he is confronted by a nationally known consultant who assures him that it is possible to measure the time that it should take a skilled person to perform elementary motions to an implied accuracy of one one hundred thousandth of a minute? Yet I have a manual in my possession which gives figures to such an accuracy and expects us to respect synthetic standards built on these figures. If at least the methods by which these figures were obtained were published, we could weigh them intelligently, but this professional knowledge is treated as a business secret.[15]

The consultant to whom I was referring was A. B. Segur, whose confidential manual had come into my possession. This manual stated times but did not give any supporting data to back up the contention of the author. Mr. Segur read the article, recognized himself, and an exchange of correspondence took place. Mr. Segur maintained that his standard times with an

[12] Maynard, Stegemerten, and Schwab, *op. cit.*

[13] Herbert Lynch, "Basic Motion Time Study," *Journal of Industrial Engineering,* Vol. IV, No. 3, August 1953, pp. 4–6.

[14] Quick, Shea, and Koehler, *op. cit.*

[15] William Gomberg, "The Relationship Between the Unions and Engineers," *Mechanical Engineering,* June 1943, p. 428.

implied accuracy of one one hundred thousandth of a minute were justified by certain kymograph studies. He failed, however, to furnish the studies, to offer to make them available, or to describe how these studies were made. It is obvious that the kymograph is an exceedingly limited laboratory instrument and findings based upon its operation can hardly be extended to industrial conditions with any degree of justification.

Holmes has published the values assigned to individual muscular reactions. However, the original data from which these time assignments were constructed remain unavailable. Until the basic methods by which these values were derived are made available, they can receive little consideration.

Barnes' system of motion-time standards draws largely upon the work of Harold Engstrom, motion-study supervisor of the General Electric Bridgeport plant.[16] Barnes and Engstrom are most conservative in making claims for their techniques. The chapter on this subject in Barnes' textbook is titled "Determining Time Standards for Assembly Operations," thus restricting the area of application. He goes on to specifiy that these times are to be assigned to individual therbligs or therblig combinations.[17] Thus his findings conform to Mundel's and his own investigations specifically that therblig time assignments vary with the position in which a therblig appears.[18] The number of possible permutations and combinations of therbligs under these circumstances make one wonder how economical the technique can be under these circumstances.

Every effort is made to leave nothing to unwarranted common-sense notions. For example, instead of assuming that if one hand could perform a therblig in a specified amount of time, then both hands performing it simultaneously would add noth-

[16] Harold Engstrom, "Development and Use of Standard Time Values," [*1940*] *Proceedings of the National Time and Motion Study Clinic* (Chicago: Industrial Management Society, 1941), pp. 45–47.

[17] Ralph M. Barnes, *Motion and Time Study* (2d ed.; New York: John Wiley & Sons, 1940), pp. 333–444.

[18] Barnes and Mundel, *Studies of Hand Motions and Rhythm Appearing in Factory Work* (Studies in Engineering, Bulletin 12; Iowa City: University of Iowa, 1938).

ing to the time of the slower hand, specific directions are given to multiply the base time by a factor of 1.40 when using both hands.[19] Thus the Mundel-Barnes conclusion on one-handed work as against two-handed work receives specific application. However, it remains highly problematical whether the investigators, as careful as they have been, have succeeded in accounting for every physical variable, let alone the other sources of variations to which we have already made reference. The important thing, moreover, is that neither Barnes nor Engstrom has made unwarranted claims for their technique outside of the area of assembly in which they have actually done their work. It remains free of attempts to apply it to subtle motion patterns, as Segur tries to do.

Holmes is not satisfied with a mere therblig or therblig combination breakdown. An examination of the chart of the movement times of body members which he presents reveals some fantastic time assignments for industrial application. For example, every mind decision is listed. It is assigned a value ranging from .0010 to .0100 with an additional remark not over .0100. Other highly questionable assignments appear, such as nerve reaction time, from eye to brain, and from knee to brain (see Fig. 12). The sense of design of the total movement is lost sight of completely. The whole spirit in which the table is presented is again along the lines of the laws of motion economy, set forth by Lowry, Maynard, and Stegemerten, to which we have referred. The unwarranted assumption is that the restriction of bodily movements to the lowest classification is, of necessity, the most economical independent of individual differences.

The directions that accompany the presentation of the chart tell a story in themselves. "Movements of operators may be determined by finding the time value for the type and length of motion made from Figure 27 [Time of Movements Chart, reproduced on page 225, Figure 12]. . . . This gives the value that the operator 'ought' to take to complete the movement. Fre-

[19] Barnes, *Motion and Time Study* Fig. 182, p. 334.

quently these values may be assigned and no further considerations are necessary."[20]

The Work Factor group has likewise attempted to reduce time assignment to fundamental bodily movements.[21] However, here an attempt is made to correct the data by the consideration of special work factors. The accuracy of the data is belied by the definition of these factors. They list these factors as an index of difficulty, skill, effort, or care involved in the performance of the manual motions. These factors are assigned as corrections to the basic data. Provision is made for four grades of correction. These are matters of judgment in assignment. Thus one of the principal claims for the superiority of standard data over other methods of fixing production standards—the elimination of the subjective factor in leveling—disappears. Again the data which went into the construction of these time assignments and allowances for correction data remain unavailable. The principal claim made for the technique is that it yields standards that are realizable.

The Methods Time Measurement system of standard data was compiled from a set of manual operations confined to drill-press work. The authors tell us that it was later found that the data applied equally well to all classes of work. The method used to develop the validity for such a key extension, however, is not touched upon in their major work.[22] The data was collated by the analysis of micromotion breakdowns of the drill press operations. The results were broken down arbitrarily into minute subdivisions very similar to the equivalent therbligs; the times that were abstracted were then leveled. The writers acknowledge that it is difficult to apply the basic information to the most difficult part of the operation, the so-called "do" and examine fundamental motions. The rest of the information falls into seven different tables: They are reach, move, turn, grasp, position,

[20] Walter G. Holmes, *Applied Time and Motion Study* (New York: The Ronald Press Co., 1948), p. 277.

[21] Quick, Shea, and Koehler, *op. cit.* pp. 102–103.

[22] Maynard, Stegemerten, and Schwab, *op. cit.*, p. 25.

disengage, and release. The investigators again state that the correct application of their system makes it possible to establish with *remarkable accuracy* the time required to perform the vast majority of industrial operations. The actual application of the system, we are warned, calls for experienced men. The principal variables in the system, it is recognized, are those particular fundamental motions which belong in any particular job. In other words, the area of controversy between the worker and the technician is over what list of fundamental motions shall be included as average for the task at hand. When all is said and done, of course, leveling has not been eliminated; it has been concealed in the basic data.

The Basic Motion Time study system developed by the firm of J. D. Woods and Gordon depends upon the same fundamental set of basic assumptions as Methods Time data. Lynch has described some of the major design innovations of this particular system. Practically all of the data was gathered from controlled laboratory experiments, in which, it is claimed, one variable at a time was introduced and its effect measured. Lynch, of course, hastens to add that no claim is made that all variables were segregated. As a result of these experiments, the Reach and Move tables are combined in the Basic Motion tables. The tables for grasp and releases are eliminated. In their stead are placed tables for precision. A number of other changes are suggested that are not important for our particular purpose at this time. It is clear that the same principal limitation which Lynch ascribes to other systems remains likewise true for Basic Motion Study. This limitation is that its application "requires judgment of a high order to decide exactly what motions could and should be employed by the average qualified operator to perform the work cycle.[23]

Gerald Bailey, a vice-president of J. D. Woods & Gordon Limited, in discussing this self-same system, acknowledges that ". . . experience in applying the technique soon indicates that a high degree of judgment is required to interpret the use of these

[23] Lynch, *op. cit.*, pp. 4–6.

TIME OF MOVEMENTS CHART

ADDED TOTALS ARE EQUAL TO THE WORK CYCLE TIME

TIME IN DEC. MIN.

BODY MEMBER MOVED	TYPE OF MOVEMENT	MOVED AT	MEASURE MOVEMENT OF	IMPULSE PRESS NERVE MIND	DISTANCE MOVED 0"TO2" 5°	4" 10°	6" 15°	8" 20°	1' 30°	1½' 45°	2' 60°	2½' 90°
FINGER	HINGE	KNUCKLE	FINGER TIP	.0015	.0017	.0021						
FINGER	SIDE	KNUCKLE	FINGER TIP	.0021	.0032							
HAND	HINGE	WRIST	FINGER TIP	.0020	.0022	.0025						
HAND	ANGULAR	WRIST	FINGER TIP	.0022	.0032	.0038						
FOREARM	ANGULAR	ELBOW	KNUCKLE	.0023	.0027	.0030	.0033	.0036	.0041			
FOREARM	HINGE	ELBOW	KNUCKLE	.0018	.0020	.0022	.0024	.0027	.0031			
FOREARM	ROTATE	ELBOW	KNUCKLE	.0020	.0022	.0024	.0026	.0028	.0032	.0038	.0045	.0053
FOREARM	TWIST	ELBOW	KNUCKLE	.0018	.0018	.0019	.0020	.0021	.0023	.0025	.0028	.0032
ARM	ANGULAR	SHOULDER	KNUCKLE	.0029	.0030	.0035	.0040	.0048	.0060	.0080	.0095	.0105
ARM	ROTATE	SHOULDER	KNUCKLE	.0032	.0034	.0039	.0045	.0054	.0067	.0090	.0107	.0118
ARM	TWIST	SHOULDER	KNUCKLE	.0020	.0022	.0025	.0027	.0028	.0029	.0030	.0032	
HEAD	HINGE	NECK	NOSE	✓	.0059	.0061	.0063	.0065	.0069	.0075	.0087	.0100
HEAD	TURN	NECK	NOSE	✓	.0051	.0053	.0055	.0057	.0063	.0070	.0078	.0090
FOOT	HINGE	ANKLE	TOE	.0024	.0027	.0032						
FOOT	ANGULAR	ANKLE	TOE	.0028	.0031	.0036						
FORELEG	HINGE	KNEE	TOE	.0036	.0040	.0043	.0047	.0052				
FORELEG	SIDE	KNEE	TOE	.0040	.0045	.0050	.0056	.0064				
FORELEG	ANGULAR	KNEE	TOE	.0045	.0051	.0057	.0064	.0072				
THIGH	HINGE	HIP	KNEE	.0036	.0038	.0040	.0042	.0050				
THIGH	SIDE	HIP	KNEE	.0040	.0045	.0050	.0056	.0064				
THIGH	ANGULAR	HIP	KNEE	.0046	.0052	.0058	.0068	.0080				
LEG	HINGE	HIP	TOE	.0030	.0032	.0034	.0036	.0038	.0043	.0050	.0058	.0065
LEG	SIDE	HIP	TOE	.0045	.0048	.0052	.0056	.0060	.0068	.0080		
LEG	ANGULAR	HIP	TOE	.0050	.0052	.0056	.0060	.0065	.0072	.0084	.0096	.0108
OPERATOR	TURN	HIP OR ANKLE	SHOULDER	✓	.0062	.0076	.0088	.0095	.0106	.0120	.0136	.0160
OPERATOR	TURN	MOVE FEET	SHOULDER	✓	✓	✓	✓	.0200	.0210	.0220	.0226	.0230
OPERATOR	BEND	HIP OR ANKLE	SHOULDER	✓	.0077	.0087	.0095	.0102	.0111	.0125	.0142	.0168
OPERATOR	SIT	HIP	SHOULDER	✓			.0180 TO .0210					
OPERATOR	STAND	HIP	SHOULDER	✓			.0220 TO .0280					
EYE	MOVE SIGHT	SOCKET	PATH ANGLE	✓	.0050	.0055	.0059	.0062	.0066	.0073		
EYE	FOCUS	GET IMAGE CLEAR		✓			.0020 TO .0040					
EYE	INSPECT	READ		✓			.0035 TO .0045					
EYE	INSPECT	SEE PART		✓			.0025 TO .0035					
EYE	INSPECT	OBSERVE (GLANCE)		✓			.0015 TO .0025					
NERVE REACTION		EYE TO BRAIN OR REVERSE		.0003								
NERVE REACTION		HAND TO BRAIN OR REVERSE		.0024								
NERVE REACTION		KNEE TO BRAIN OR REVERSE		.0026								
NERVE REACTION		FOOT TO BRAIN OR REVERSE		.0030								
NERVE REACTION		REALIZE CONTACT		.0010 TO .0040								
NERVE REACTION		HEAR OR SMELL		.0025 TO .0040								
MIND DECISION		MENTAL PROCESS		.0010 TO .0100				NOT OVER .0100				

NOTE:— USE CIRCULAR MEASURE FOR UNDERLINED VALUES

				PER STEP AFTER 5 STEPS	1 STEPS	2 STEPS	3 STEPS	4 STEPS	5 STEPS	8 STEPS	10 STEPS
OPERATOR	WALK										
OPERATOR	WALK	HIP	6" STEP		.0160	.0260	.0350				
OPERATOR	WALK	HIP	12" STEP		.0210	.0330	.0440	.0530	.0610		
OPERATOR	WALK	HIP	18" STEP	.0070	.0240	.0370	.0490	.0580	.0640	.0850	.0990
OPERATOR	WALK	HIP	24" STEP	.0075	.0260	.0400	.0530	.0630	.0720	.0945	.1150
OPERATOR	WALK	HIP	30" STEP	.0080	.0270	.0420	.0560	.0670	.0770	.1010	.1170

FIGURE 13*

* Walter G. Holmes, *Applied Time and Motion Study* (New York: The Ronald Press Co., 1938), Fig. 27, p. 244, reproduced by permission of the publisher.

data—to know where to interpolate or modify and what the results of such action will be."[24] Thus Basic Motion Time Study is not distinctive in this respect. Time derived from Basic Motion Time Study for the marble operation in the S. A. M. rating film is very close to the result which the S. A. M. investigator obtained. The normal time under B. M. T. is .0232, the translation of the S. A. M. result into B. M. T. units is .0239. We have reviewed our estimate of the S. A. M. films already and we note that the B. M. T. system is derived from leveled data. We are inclined to accept Lynch's explanation that such close correlation can be linked to chance.

The evidence in support of these systems is that "they work." We have dealt at some length with this criterion of scientific validity, and there would be little point in repeating ourselves here. However, this criterion of empirical workability has been opened up by Davidson.[25] Davidson indicates that he is only interested in time study as a viable pragmatic technique. He does not want to become involved in any more substantial criterion of accuracy. He then describes some experimental work of R. W. McGuire, a research fellow of Ohio State University.

Mr. McGuire was interested in testing this proposition: If one of several contemporary standard data systems is accurate, then the others cannot be accurate. The only system for which this test could be made was Holmes' Methods Time Measurement and Work Factor. The others did not publish their tables of data (except for Basic Motion Time Study which was not yet in existence at the time of the publication of the Davidson manuscript). McGuire and Davidson selected comparable elements from these systems of minimum complexity to avoid the complications of correcting factors. It therefore could not be argued that inexperienced people were using the systems.

The data which was used is reproduced in the table below.

[24] Gerald B. Bailey, "Basic Motion Time Study," *8th Annual Bulletin,* Conference Proceedings of the Society for the Advancement of Management and the American Society of Mechanical Engineers, April 1953, p. 91.

[25] Harold O. Davidson, *Functions and Bases of Time Standards* (Columbus, Ohio: American Institute of Industrial Engineers, 1952), pp. 142–164, 333–348.

The primary study to which this data was subjected was a "two factor analysis of variance to test the hypothesis that differences in times among basic elements and among distances within elements are not statistically significant when compared to the difference of times among the various systems of standard data."[26]

This treatment of the data led the investigators to conclude that the differences found could not possibly be attributed to chance and that "The general proposition is confirmed." If one

TABLE VII

Comparison of Basic Elements and Times from Three Systems of Standard Data

Basic Element	Distance (inches)	Basic Time—Minutes		
		Work Factor	M-T-M	Holmes
Finger	1	.0016	.00108	.0017
	2	.0017	.00222	.0017
	3	.0019	.00300	.0017
	4	.0023	.00366	.0021
Hand	1	.0016	.00108	.0022
	2	.0017	.00222	.0022
	3	.0019	.00300	.0022
	4	.0023	.00366	.0025
Forearm	45°	.0017	.00576	.0032
	90°	.0023	.00894	.0045
Arm	1	.0018	.00108	.0034
	2	.0020	.00222	.0034
	3	.0022	.00300	.0034
	4	.0026	.00366	.0039
	6	.0032	.00420	.0045
	8	.0038	.00474	.0054
	12	.0046	.00576	.0067
	18	.0055	.00738	.0090
	24	.0063	.00894	.0107
	30	.0070	.01050	.0118
Foot (Hinged Movement)	1	.0020	.00510	.0027
	2	.0022	.00510	.0027
	3	.0024	.00510	.0027
	4	.0029	.00510	.0032

[26] Davidson, *op. cit.*, pp. 156–157, 333.

Basic Element	Distance (inches)	Basic Time—Minutes		
		Work Factor	M-T-M	Holmes
Trunk (Bend)	1	.0026	.00174	.0077
	2	.0029	.00174	.0077
	3	.0032	.00174	.0077
	4	.0038	.00174	.0087
	6	.0047	.00174	.0095
	8	.0054	.00174	.0102
	12	.0066	.00174	.0111
	18	.0080	.00174	.0125
Leg (Hip to toe)	1	.0021	.00426	.0032
	2	.0023	.00426	.0032
	3	.0025	.00426	.0032
	4	.0030	.00426	.0034
	6	.0037	.00426	.0036
	8	.0043	.00570	.0038
	12	.0052	.00858	.0043
	18	.0063	.01290	.0051
Leg (Hip to toe) Side Movement	1	.0021	.00108	.0048
	2	.0023	.00222	.0048
	3	.0025	.00300	.0048
	4	.0030	.00366	.0052
	6	.0037	.00420	.0056
	8	.0043	.00534	.0060
	12	.0052	.01020	.0068
	18	.0063	.01236	.0080
Walk (general case, unrestricted)	1 pace	.0150	.0102	.027
	2 pace	.0260	.0240	.042
	3 pace	.0360	.0360	.056
	5 pace	.0520	.0510	.077
Eye Fixation		.0020	.00438	.0020
Visual Inspection		.0030	.00438	.0025

of the systems is accurate, the other two cannot be. Though Work Factor and M. T. M. differed less than any other pair, the disparity between them showed that this difference too could not be attributed to chance.

A later study by professors L. C. Pigage and L. I. Reis, both

of the University of Illinois, utilized a different method to prove the same conclusions reached by Harold Davidson.

Pigage and Reis tested a hypothesis proposed by A. M. Lang.[27] Lang had suggested that the various predetermined motion time systems could be reconciled by determining a time-distance-control surface for any motion. "The 'control' dimension represents such factors as degree of precision, weight handled, and muscular control required. 'Distance' represents the length of the motion, and the 'time' is, of course, the time required to perform the motion."[28]

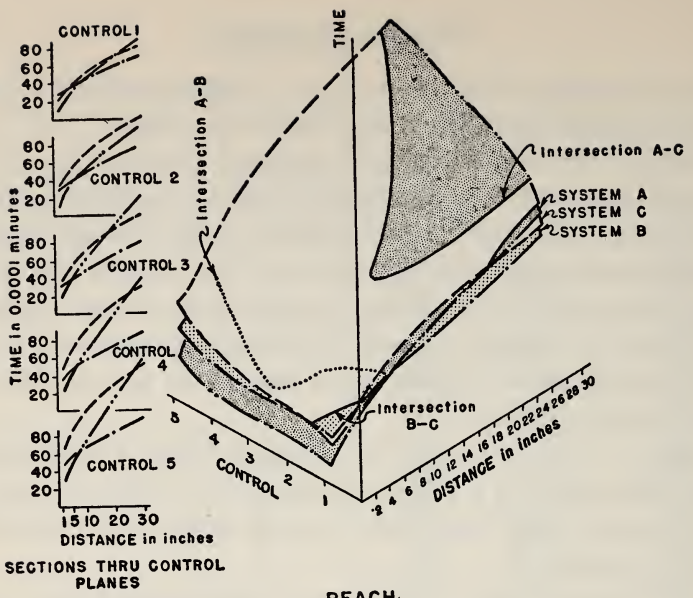
Pigage writes that Lang had suggested that if a slice were taken through such a common surface that different time-distance curves would vary with the particular point at which the slice was taken.

The different values for the same fundamental motion given by different predetermined time systems could then be explained in terms of a different slice at a different control point through the surface. Pigage and Reis write:

> To test whether a common surface exists, the authors of this paper first factored out, in so far as possible, the differences between the bases, using the published statements as to the level of worker performances represented by three of the commercial data tables. Then several levels of control which could be matched closely from system to system were selected, and the data for three motion categories plotted on isometric axes of time, distance, and control. The results are shown in Figures 13 (reach), 14 (move), and 15 (forearm swivel).
>
> It will be noted that in each category, the data appear not as one surface, but as three separate and distinct surfaces. Furthermore, in most cases, the cross-sectional time-distance curves are dissimilar in shape. Intersections of the various surfaces are irregular. Time value differences at any given point may run as great as 100 per cent.

[27] A. M. Lang, "Methods-Time Measurement," Proceedings of the Eighth Annual Time Study and Methods Conference, S. A. M. and A. S. M. E., New York, 1953, pp. 101–102.

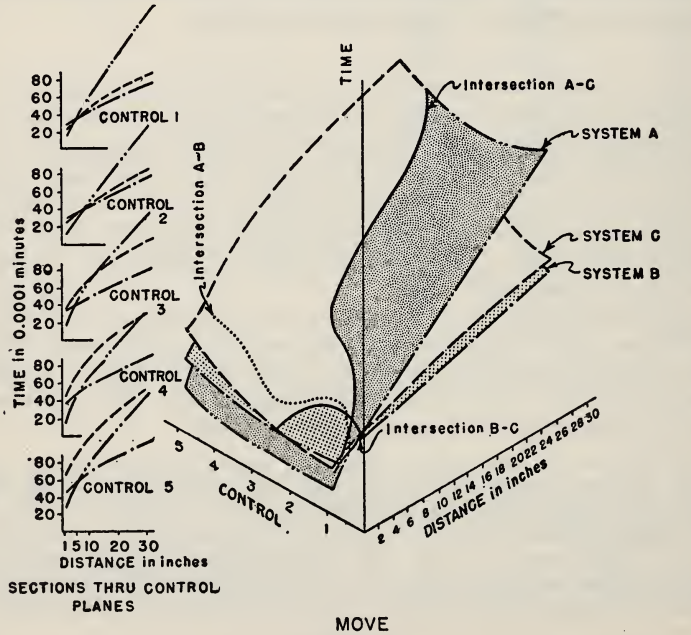
[28] L. C. Pigage and L. I. Reis, "The Incompatibility of Predetermined Time Systems," paper prepared by the A. S. M. E. at the Fall 1954 meeting in Milwaukee, Wisconsin, p. 2.



REACH

FIGURE 14

Isometric representation of "reach" time-distance-control surfaces for each of three common systems of predetermined times, based on published data.



MOVE

FIGURE 15

Isometric representation of the "move" time-distance-control surfaces for each of three common systems of predetermined times, based on published data.

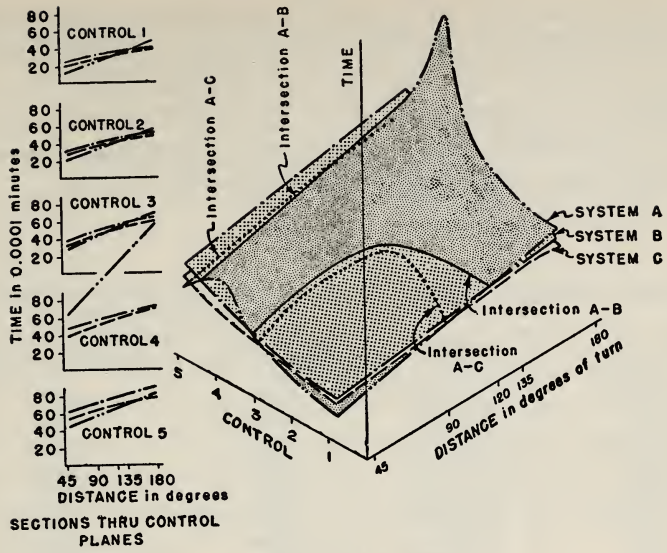


FIGURE 16

Isometric representation of the "forearm swivel" time-distance-control surfaces for each of three common systems of predetermined times, based on published data.

From this evidence, it appears that for the three systems studied, at least, it is not possible to say whether all are derived from one basic time-distance-control surface stemming from the movements of operators performing work. If they do come from such a surface, then it appears that there were serious errors in the derivation of the data from the basic information into the published systems. If they are each equally accurate, yet do not come from such a time-distance-control surface, then considerable doubt is cast upon the existence of a fundamental time-distance-control surface and therefore upon the whole concept which serves as the foundation for universally usable determined times.[29]

The authors[30] of the Methods Time Measurement system have not offered very substantial evidence in support of their system. M. T. M. finds support in an analysis which its authors made and a study undertaken by K. C. White of Cornell University.

A review of his evidence reveals that:

[29] *Ibid.*, p. 3. (The figures are from page 6.)
[30] Maynard, Stegemerten, and Schwab, *op. cit.*, p. 130.

The authors attempt to validate their system by comparing the results of 27 time studies by the leveling method with the results obtained by Methods Time Measurement for the same 27 different operations.[31]

The M. T. M. organization offers the following table as proof of M. T. M. validity. It is a comparison of results for operation obtained by time study with those obtained by M. T. M.

TABLE VIII

Part No.	Department	Leveled time, dec. min.	
		Time study	Methods-time data
E 21879	Forming	.0810	.0828
E 9113	Forming	.0950	.0887
E 26312	Forming	.0461	.0422
E 17780	Forming	.0428	.0400
E 2518	Forming	.3474	.3504
E 24060	Forming	.0330	.0336
E 25837	Switch	.0954	.0942
E 2486	Sundries	.1800	.1788
6174	42	.1452	.1452
6197	42	.1260	.1320
5612	Switch	.1210	.1220
6208	43	.6798	.6780
E 24769	Forming	.0640	.0640
E 4646	Forming	.0570	.0570
E 25921	Receptacle	.1080	.1086
C 2040	Switch	.1680	.1740
C 10	Switch	.0588	.0594
87145	43	.1260	.1200
73752	Sundries	.1247	.1135
Cat. F56	Switch	.1420	.1432
F 6233	Switch	.1060	.1056
E 25993	Switch	.1260	.1218
FS 5	Switch	.0730	.0733
E 3854	Forming	.0680	.0620
E 24060	Forming	.0330	.0330
E 23975	Forming	.0343	.0381
E 20486	Forming	.1800	.1800

Then they conclude:

The total time allowed by Time Study was 3.4615 minutes. The total time allowed by Methods Time Measurement procedure was 3.4414 minutes. This is a difference of less than six-tenths of one per cent.

[31] K. C. White, "Predetermined Elemental Motion Times," speech delivered at the 1950 annual A.S.M.E. meeting.

Inasmuch as both sets of figures include leveled data, it is not clear exactly what this is supposed to prove. Then again individual differences for the 27 operations should have been paired and their deviations squared and totaled. Totaling respective times and comparing over-all totals is misleading because positive and negative errors cancel out each other. This leads to attributing a much closer correlation to the data than actually exists.

Professor White took new films in eight different companies on a series of operations ranging from small assembly and riveting operations to machine handling operations. He confined his study to breaking down the work elements by the same methods used by the M. T. M. group including leveling. His object was to determine whether or not the basic M.T.M. data was reproducible. He concluded from his examination of Methods Time Measurement that:

> The data collected so far appears to point toward the practicability of defining work elements in terms of fundamental elements of motion common to a wide range of industrial activity and the establishment of times for these elements. . . . In summary the check studies have consistently ranged within approximately plus or minus one per cent of the M.T.M. times. For individual elements time differences between check times and M.T.M. times in the order of 0.1 to 0.5 T.M.U.'s (.00006 to .0003) minutes are not uncommon and, as evidenced in this paper a few are larger. . . . No reason is now apparent why continued study should not result in reconciling the differences that do exist.[32]

Both Mundel and Nadler have questioned the validity of the White study. Nadler states:

> This Cornell University work arrived at the conclusion that the systems can work and that the data of the system studied are essentially accurate. However, there seems to be a lack of correlation between the facts presented in the report and the conclusions drawn.
>
> 1. The Cornell University researchers admit that leveling or rating is not accurate. One conclusion states that "since a single level of performance is represented," the standards are consistent. Where did such a single level of perfomance come from when the rating procedure is not accurate?
>
> 2. The study adds up the check study values for given motions and

[32] K. C. White, *op. cit.*, p. 10.

adds up the same motion values from the procedure under study. The difference of totals is 0.9%. This is somewhat misleading. Why not add a few more values to both sides? The percentage difference will be decreased because the calculations will deal with a larger number in the denominator.

The true percentage differences are obtained by subtracting one value from the other for a given motion, converting this subtraction to a per cent by dividing by one of the values and then calculating the variability based on these per cents from all of the motions.

3. The report states that there is 7½% error 75% of the time in the standards set or, turning it around, there is *more* than 7½% error 25% of the time, and yet the conclusion states that standards set by the system are accurate. At this point the Cornell University work substantiates the conclusions of this paper. The variability of the check study time values was almost exactly the same as that calculated by our studies.

4. Another statement mentions that the "check studies have consistently ranged within approximately plus and minus 1%" of the predetermined motion time stem being checked. However, the data in the back of the report which was presented for the simplest of all activities, transport empty and transport loaded, show that only 20% of the values are within 1%. Actually, there was a difference from the studied system's values of greater than 4.6% to 6.7% one-third of the time. This error of nonreproducibility of time values supports the conclusions of the research and analysis work presented in this paper.

5. Another statement in the report indicates that although there were some high errors, these would be eliminated by combination with other motions. As indicated before, this is not the case. The concept referred to here is well known to those familiar with statistical quality control, in relation to combination of tolerances in assembly work.

6. A statement is made that context therbligs have some effect on times but that context can be taken care of by the addition of a new motion. This could possibly be true, but how could this be done in view of the results of the work which was done for the Navy and the experiments which were done at Washington University?

7. Tables are presented giving the leveling factor and the time for each distance of movement. The leveling factor has correlation of .98 with the distance traveled. This is rather exceptional, since research work has shown the pace or leveling factor should have no relationship to distance.[33]

The most important problem, however, remains, and that is, can any fundamental motion time system be scientific? We have referred to the findings of Barnes and Mundel which state that the time value of a therblig is a function of the sequence

[33] G. Nadler, "Critical Analysis of Predetermined Motion Time Systems," *Proceedings of the National Time and Motion Study and Management Clinic* (Chicago: Industrial Management Society, 1952), p. 28.

within which it appears. This statement is intimately linked with the possibility of whether or not fundamental motion times can constitute an additive set. The only evidence which we have seen that would partially substantiate a positive answer is offered by Stilling of the University of California.

The very important restrictions the author imposes upon his results make one wonder how significant the whole experiment was. The conclusions state, among other things, that "The elimination, and as a consequence, the partwise treatment of motion elements do not change the collective times of the remaining elements provided:

1. the relative sequence of the elements remains unchanged.
2. the elimination does not destroy the basic geometric movement pattern of the task.[34]

The relative weight of evidence for the additivity of motion elements advanced by Stilling is overwhelmed by the weight of evidence against any such possibility given by Abruzzi and Nadler.

We refer the reader once again to the results of Adam Abruzzi's work on macroscopic standard data systems. The same findings apply even more strongly to the microscopic standard data systems. The same network of relationships that was found among time study elements is found to be even more pronounced for relationships between so-called fundamental motions. Abruzzi supplemented the experimental designs to which we have already made reference with separate laboratory studies for fundamental motion times. These laboratory studies revealed:

> . . . that there is no justification for the rather common belief that the times required to perform so-called fundamental motion times are constant within reasonable limits. What actually happens is that constant readings are obtained only when the motions (or elements) are not much larger than the smallest unit of measurement.[35]

[34] Donald Stilling, "A Study of the Additive Properties of Motion Element Time," *Proceedings of the fifth annual meeting of the Industrial Engineering Institute,* University of California, 1953, p. 43.

[35] Adam Abruzzi, *op. cit.,* p. 23.

He recommends, therefore, that the smallest fundamental motion that should be measured should be at least five times as long as the smallest measurement unit. He finally concludes:

> Accordingly, the likelihood of developing meaningful motion (more properly motion-group) standard data is even more remote than it is for elements.[36]

These findings are also confirmed by Gerald Nadler.[37]

Nadler poses this fundamental question: "Are small motions separable in a quantitative manner?" That is, can the time value for a particular element be separated from a job?

Nadler has designed three experiments, the results of which indicate that the answer to this question is no!

Experiment number one consisted of a simple job. An operator sat at a table which could be tilted at will. The angle of tilt ranged from 0 to 90 in 15 degree increments. The operator was instructed to move his hand from a fixed starting button to the switch a fixed distance away, turn the switch through a 30 degree angle and return to the starting button. This is the familiar therblig transport empty. The time for the therblig showed a significant variation of 16 per cent for the different angles of tilt at which the board was set.

Nadler then assumes that this objection could be overcome by amendment of all of the predetermined motion time systems, none of which provide for this variable at the present time, thus modifying the experiment. This time the table is fixed at an angle of 45 degrees, but this time variations were measured for the transport empty therblig for three different switches at the same location, a definite stop type, a click type, and a free moving type. All three switches required the same energy to move them. The time differences for these three different arrangements, after eliminating the actual time for moving the knob, were 37 per cent. Again, he assumes that a correction factor could be introduced for this variable, though the economic cost of designing such a system which would include all of the

[36] *Ibid.*, p. 24.
[37] G. Nadler, *op. cit.*

permutations and combinations of corrective variables for this purpose seems prohibitive.

The fundamental operation was then modified in still a third experiment. A new button was added half way between the old button and the switch. A new operation, called operation B, was now added. It included moving to the knob switch, turning it 30 degrees, moving to the new button, depressing it, moving back to the switch, turning it 30 degrees, and the return to the home button. The time for the single empty transport through the full space between the original button and the switch was 12 per cent different from the sum of the two smaller transport empties across the same distance.

The results of these experiments led Nadler to conclude that: "Now even if there were a predetermined motion time system which could account for the angle of tilt and the context or terminal therbligs, they still could not account for the variation on context therbligs caused by the addition or elimination of a new motion."

Nadler thus confirms our hypothesis that a therblig's time is affected by the preceding and following therblig and that the therblig times do not constitute an additive set.[38]

This controversy over the validity of standard data was brought to a head at a general conference on methods of establishing motion time data called by the American Standards Association, January 23, 1953. The conference was initiated as a result of a letter sent to the American Standards Association by Mr. H. B. Maynard on behalf of the Methods Time Measurement Association, pertinent abstracts of which follow:

. . . I am writing to you formally to request the American Standards Association to initiate a project designed to establish standards in the field of predetermined motion times.

At the present time, there are several systems which use predetermined motion time standards, the best known of which is probably the methods-time measurement procedure. This procedure was fully described in a book published in 1948 entitled *Methods-Time Measurement* by Maynard, Stegemerten, and Schwab.

[38] G. Nadler, *op. cit.*

The M.T.M. Association since its inception has urged that the proponents of all systems of predetermined motion time standards get together and develop one set of universally acceptable standards.

In attendance at the conference were representatives of organized labor, the technical societies, and trade associations. The current attitude of organized labor toward these techniques is perhaps best understood by extracts from the stenotyped manuscript that summarizes the conference proceedings.

Mr. Stegemerten, one of the founders of the M. T. M. system, went on to add that:

The Society for the Advancement of Management had appointed a committee for the purpose of standardizing and bringing about uniformity in the rating of performances of operators being studied, with a view to determining a time standard for the performance of operations. Mr. Maynard and Mr. Stegemerten had been members of the S.A.M. committee. Their work on this committee had led to the idea of setting up a set of predetermined time standards as further developed in the book on Methods-Time Measurement.

Mr. Stegemerten further added:

The area of greatest controversy in setting standards of this kind had been in the rating of performances, and it had been felt that with a procedure such as the proposed predetermined motion-time data, this area of controversy would be eliminated.

After the approach to the problem, as now sponsored by the M.T.M. Association, had been announced, there had been introduced a number of other approaches which more or less deviated from the original M.T.M. System. In the opinion of the M.T.M. Association, its purpose would be defeated if such disagreement between systems continued. Therefore, the M.T.M. Association had developed the idea that it would be well if all those interested in furthering the scientific approach to the establishment of work standards could agree on a single approach based on a unified standard procedure for predetermining operation times.

Mr. Winslow, an officer of the M. T. M. Association added that:

There were thousands of engineers today who had been trained in the use of predetermined times. Great value could be obtained from the various systems in use. Many of these, Mr. Winslow stated, were not even known to him, but their authors certainly should be invited to make a contribution to the general project.

The American Federation of Labor representative stated that:

The A. F. of L.'s position was that the A. S. A. should not concern itself with this field because there was nothing to standardize as yet.

He added that:

He did believe that the labor movement and the engineers should get together under their own auspices and hammer these things out. In fact, he had made a suggestion to this effect when the S. A. M. rating film was first publicized, but the opportunity was never accepted. This was when labor wanted to question the value of the rating films developed by the S. A. M. He believed that these matters should first be straightened out within the societies engaged in research in this field before the problem was brought up before the A. S. A. in an effort to set up an American standard.

Mr. Weinberg, the representative of the C. I. O. stated that:

. . . Mr. Winslow had repeatedly stated that there were thousands and thousands of engineers and hundreds of companies using the proposed system. Well, there were thousands and perhaps millions of people selling patent medicines of all kinds and the question arose whether the A. S. A. would be operating in its proper sphere if it attempted to standardize the terminology of the bottles these people tried to sell to the public. Also, there were thousands of astrologists, who, using different systems, came forth with different results, and there were people inventing machines for perpetual motion. Did this mean that there should be standards in the field of astrology and for the development of perpetual motion machines? Before any attempts were made to develop standards in the field of motion study, it seemed to me that an investigation should be made whether there was anything worthy to attempt to standardize. In this respect, those present might become acquainted with the work done by Dr. Abruzzi and Dr. Davidson.

Dr. Davidson went on to say that

he was present in a dual capacity. In representing Ohio State University, he was supposed to be one of the academicians. However, he also represented the American Institute of Industrial Engineers, a professional organization of individuals having the responsibility for choosing the methods they will use, in the same way as a doctor will do this.

Dr. Davidson then added:

. . . A pertinent question was what was meant by a "standard"? He said there were things that might be called "natural standards," such as physical constants. These could be determined by scientific methods. Perhaps the approach to motion-time standards was predicated on the assumption that there was such a thing as a basic, fundamental motion. However, he believed that in the opinion of industrial and experimental

psychologists, there was so far no evidence that there existed any fundamental basic motion. Therefore, the classification should be regarded as an arbitrary one. As long as people agreed as to what the term meant, they could communicate but the term should not be regarded as defining a fundamental thing.

It appeared that the A. S. A. should concern itself with standards that might be called "arbitrary" standards, these were set up on the basis of a consensus of all people interested and were desirable. Such arbitrary standards had been established where there was no natural standard, and where it was impossible to apply scientific methods. Therefore, Dr. Davidson felt that some of those present who had spoken earlier had not expressed themselves correctly when they described their system as a scientific approach. Rather, the methods under discussion were used because there was no scientific approach. It was a pragmatic approach.

The American Federation of Labor representative then challenged:

It might be well to state plainly the question: "Why do you want an American Standard?" Could the reason possibly be that it became embarrassing when a trade unionist declared that all of the proponents of systems had different situations, and all of them gave the wrong answers? If all proponents could get together and give the trade unions one answer, this would make it easier for the proponents. [The implication was that it sounded like a meeting to propose the ground rules for a conspiracy to conceal valid scientific differences from the public.]

The conferees finally voted 16–3 to reject any American Standards Association project at this time in the field of predetermined motion time.

What does this add up to in terms of union policy when confronted by these predetermined motion time systems? The trade union point of view was summarized in the same address at the University of California to which previous reference was made.

Does this mean that trade unions will reject all systems of standard data out of hand? Not at all. We are willing to recognize that they are a rather poor empirical attempt to develop a modus vivendi upon which both management and labor can reach agreement. However, under no circumstances would we permit ourselves to be bound solely by the technology of these systems in dispute cases. They at best stabilize a human relationship. They are useful provided we recognize that the failure to achieve a rate developed under standard data does not indict the worker; the rate may very well point up the limitations of the technique.[39]

[39] William Gomberg, *op. cit.*

A look backward over standard data would indicate that on the macroscopic level it possesses the virtue of a certain limited consistency in that workers know more or less in advance what times are assigned to certain work elements. This type of consistency should not be confused with the consistency implied when balanced rates are discussed. In the latter case, the assumption is made that all elements are so accurately set that when added into a job, every job so added will present the same earning opportunity under a wage incentive payment plan.

Too little is known about the origins of the data of most microscopic standard data plans. On the basis of inconsistency among themselves, we can conclude that they are very dangerous to use. All too often correction factors are rationalized after it is demonstrated that the standard set is too tight. If we could be sure of the initial choice of the correction factors, then perhaps the argument that the system has at least empirical validity might be defensible.

The determination of all the weaknesses of time study has been a comparatively simple matter, but now with what do we replace it to create a better suited method? How are we to measure a fair day's work? How are we to set production standards, whether it is for measured day work, for an incentive payment plan, or for that matter, to build a production schedule?

The answer, of course, is that we do not desire to do away with time study. We have merely established its past limitations as a technique. Many of these limitations remain in the qualitative stage and so we are unable to set up quantitative limits within which the measurements will vary. This should not be cause for discouragement, as an examination of collective bargaining practice in Chapter 17 will indicate.

CHAPTER 16

A Look Backward

To recall:

Chapter 1 opened with a comparative history of the process of collective bargaining and the technique and philosophy of scientific management. The roots of the conflict between the pioneers of the two movements were traced and a basis for the peaceful coexistence of the two movements was developed.

Chapter 2 opened with a formulation of the problem of the study for rate-setting purposes as it has developed in the course of collective bargaining. The principal problem that required a solution was whether or not the residual accuracy of the time study technique under the very best conditions was close enough to warrant its use as a complete guide to setting the rate structure under collective bargaining. The successful setting of such a range of accuracy, it was indicated, might lead to the application of time study techniques to a wide variety of functions including that of an index of productivity.

Chapter 3 was devoted to an examination of the nature of the scientific method in the light of the needs presented by time study. We noted that our principal purpose in developing a time study system was to provide a systematic way of thinking about the technique so that prediction about future production rates from individual studies would have some rational basis. The

various areas of measurement were noted, ranging from complete determinacy to complete random variation. Arbitrarily dividing this range into three areas, the area of random variation, the area of statistical laws, and the area of exact physical laws, we posed the time study question as essentially the solution of the problem of determining into what area of measurement time study fits.

Chapter 4 was devoted to the erection of a time study system postulated upon the acceptance of the statistical view of nature. Utilizing the thinking of the quality control engineers and some of the tools perfected by the mathematical statisticians, we developed the structure to fit our objective. This objective was to define a state of statistical control for a time study system in terms of a physical chance cause system and the quantitative characteristics of the sequence of observations produced by such a cause system.

Postulating an idealized condition, we found that the use of control charts provides a tool for indicating the existence of a state of statistical control in terms of the quantitative characteristics of a sequence of observations. A positive test does not guarantee the certainty of the existence of such a state. A negative test would be a certain indication that such a state did not exist. This serves as a criterion to test whether or not the person under observation is ready to be time studied. That is, it provides a test of the uniformity of method being used and of the consistency of the rate.

We found that in the event such a state of control did exist, then a production rate standard could be fixed by specifying the population arithmetic mean from the sample means.

The limits of variation of the production rate standard are set by the population standard deviation estimated from the sample standard deviations.

The coefficient of variation multiplied by three under these conditions provides a criterion of the number of readings required by the time study. This condition is arbitrarily set so that three times the coefficient of variation is less than five

per cent in order to satisfy the requirements of collective bargaining.

Thus Chapter 4 ends with the conclusions that may be drawn from a sequence of observations where a physical constant chance cause system is assumed, thus establishing a state of statistical control.

Chapter 5 analyzes the characteristics of the time-measuring mechanisms in time study. Inasmuch as the variation of the readings in a time study are just as important as the final arithmetic mean in drawing a set of conclusions, it is demonstrated that the stop watch is almost useless if we are to keep within a reasonable range of accuracy. The marstochron is superior. The dangers of snap-back readings as against continuous times are then analyzed and the reasons given for preferring the continuous method. After establishing the relative accuracy of the time-measuring instruments, an analysis is made of the physical constant chance cause system.

Chapters 6, 7, 8, and 9 are devoted to an examination of the cause system at work in industry in order to determine a priori what the possibilities are of establishing a constant chance cause system that would permit extension of conclusions from our idealized picture in Chapter 4 to the industrial environment.

Chapter 6 is devoted to an examination of the mechanical sources of variation. It was found that these sources of variation fell into two classes: (a) those that interfere with the regularity of the repetitive operation and present no problem except a statistical analysis to account for their occurrence; and (b) those whose effects are more subtle and are suspected of affecting the fatigue accumulation of the worker. The effect of variations from this source remains indeterminable.

Chapter 7 is devoted to an analysis of the physiological sources of variation. We find that present methods of measuring fatigue are largely tautological, defining fatigue in terms of reduced output. The change in the approach of Hoagland and Pincus and the use of the ketosteroid index of physiological tax is described. More work must be done before we can determine

what the optimum ketosteroid output should be during the working day. Hence, here again we have an indeterminate source of variation which engineers are unable to measure, but which of necessity must vitally affect the chance cause system which must remain constant if time study is to be a valid technique. The ketosteroid approach to fatigue has the virtue, however, of opening the road to a rational solution of this problem.

Chapter 8 takes up some of the assumptions upon which the theory of the one best method is based. It is shown that many of the psychological assumptions behind this method remain undemonstrated and that, as a matter of fact, there is some evidence that within certain limits there is no such thing. The chapter then goes on to describe the purely emotional effects of the person's state of mind upon the production process.

Finally, in Chapter 9, an examination of the sociological forces at work demonstrates that their principal effect is on this state of mind, which seems to be the biggest factor in determining what the level of production *should* be.

The conclusions of the first nine chapters are summarized in Chapter 10, where it is indicated that time study phenomena fit in between Areas I and II (Fig. 1, p. 48) of our measurement diagram. Finally, the rate of production is interpreted as a function of the motivating drive. This phenomenon is pictured in a diagram on page 143.

The conclusion of Part One would indicate the probability of establishing a physical constant chance cause system. Inasmuch as a time study has a rational meaning only when it is interpreted as a sample of a parent population subject to a constant chance cause system, the limitation of the possibility of such a constant chance cause system in our modern industrial environment makes it highly doubtful that there is such a thing as scientific time study.

Part Two begins our examination of present industrial time study practice.

Chapter 11 is concerned with the standardization of the job.

It is noted that every industrial practitioner subscribes to the doctrine of the one best method. Virtually all, with the exception of Barnes, subscribe to the undemonstrated assumption that job elements compose an additive set.

Chapter 12 indicates the various methods of drawing inferences from a body of data. There are almost as many statistics derived from the sample as there are investigators drawing them. Reasons are noted for our insistence upon the use of the arithmetic mean.

Chapter 13 is devoted to an examination of the industrial time study man's concept of normal performance and notes, first, the wide variation in the concept of normal and, second, the lack of rationale in modern leveling and rating techniques as objective measurement.

In Chapter 14 the much heralded S.A.M. project on the rating of time studies was examined. It was discovered that the claims made for the comparative closeness of rating results of American time study practitioners were not warranted by the results of the investigation. The basic design of the investigation was faulty. The investigation's principal usefulness arose from the disclosure of the completely different and inconsistent rationales underlying the structure of the different rating systems.

Chapter 15 reviews present methods of setting rates by means of libraries of standard data. It indicates the wide range of variation in times assigned to the same very simple therbligs. Furthermore, it makes clear that standard data are built upon the questionable assumption that the data elements compose an additive set.

Part Two has made clear that thus far modern industrial time study techniques can make no claims to scientific accuracy. *They are at best empirical guides to setting up a range within which collective bargaining over production rates can take place.*

CHAPTER 17

A Look Forward

In Chapter 4 it has already been indicated how the time study technique may be used to set up the range within which collective bargaining over production rates may take place in accordance with sound statistical thinking. However, the problem that still remains is how rate disputes that fall within the range set up by the time study technique are to be settled. The establishment of the fact that time study is of a statistical nature indicates at once the limitations of its use as the sole guide to the resolution of rate grievances, whether they arise under the administration of wage incentive payment plans or from the setting of quotas under measured day-work plans. Specifically, if there is a five per cent difference between the workers' request and the management's offer when the time study technique indicates a residual accuracy for some production standard exceeding this difference, how is such a dispute to be resolved? It is immediately apparent that once we have used the techniques described in Chapter 4 to set up our accuracy range, time study thereafter cannot be used to resolve the dispute.

This limitation need not discourage industrial engineers unduly, however. The range of accuracy of time study technique compares favorably with those of other fields of engineering, but greater accuracy is required in this field than elsewhere.

Writing some time ago in the *Industrial Engineer*, I put the case this way:

The criterion of workability in industrial engineering is much more difficult to attain than in the physical branches of the profession despite similar levels of accuracy. For example, let us compare the accuracy of setting a time study standard in industrial engineering with the accuracy of determining stresses in beams [a problem from the field of civil engineering]. . . . Both techniques compare favorably as to relative accuracy, but there is a wide discrepancy as to criterion workability.

The civil engineer guarantees the workability of the bridge he designs by multiplying his results in important designs . . . by a safety factor in the neighborhood of twelve. Can you imagine what would happen if the industrial engineer attempted to use a safety factor of two, not twelve. . . . [?][1]

Carl Barth summed up the case for time study in his reply to the Gilbreths, an extract from which we have already quoted:

I cannot see but that it is as scientific as a majority of the determinations made in other fields of engineering. . . . Such determinations are in the end based on certain assumptions that we know to be only approximately correct. Thus we never know the exact ultimate strength or elastic limit of any material we intend to put into an engineering structure; but we nevertheless assume something definite, that is, we guess, and trust to luck that our guess is near enough correct to serve for practical application. . . .[2]

Certainly this philosophy opens to question the Hill-Hook position that the administration of these standards is inherently a unilateral function of management. Old hands at collective bargaining on management's side of the fence have been aware of this limitation for a long time. For example, Mary Gilson remarks:

Since wages and standards of production are so closely related, workers were naturally anxious to be "in on" time studies. The prolongation of a bitter strike in the automobile industry in 1939 was partly due to the desire on the part of the union to have the timing of operations and the setting of standards arrived at by mutual discussion. The management refused to grant what seems to me in the light of my own experience a reasonable request. They declared that production standards were the

[1] Gomberg, "Labor Examines Time Study Methods," *Industrial Engineer*, March 1944, p. 1.

[2] *Symposium on Stopwatch Time Study*, p. 110.

exclusive prerogative of management. That workers should not only be informed of methods of timing but that production speed should be jointly determined by management and workers seem to me a logical and necessary step in the democratic conduct of industry.[3]

The logical solution of the rate setting problem consists in this recognition of rate setting as essentially a bargaining arrangement that takes place in the factory when new products go into production. The function of the time study engineer is to keep this bargaining within rational bounds. However, the attempt to offer time study as a substitute for bargaining is questionable. It is an attempt to impose a task upon the technique which it is not equipped to handle.

Frederick Harbison and John Coleman describe the nature of joint time study committees in their examination of collective bargaining relationships in 18 companies. They write:

> The two time study or job evaluation committees included in this survey were instituted by companies which were looking for ways to promote acceptance by unions and workers of incentive systems. Management looked upon these committees as devices to remove the mystery surrounding time study techniques and thus to enlist worker support of "scientific" job evaluation and rate setting. The unions, which played passive roles in the establishment of the committees, were inclined to believe that their participation in the time study departments was a means of protecting workers' job interests under an incentive system. From the available evidence, we concluded that there was little difference between the rates arrived at by joint time study and those which management set on its own initiative. But there was a wide difference in the acceptance of these rates within the plant and hence in the effectiveness of the incentive system.
>
> . . . . .
>
> The most obvious benefit of both the time study committees we examined was to eliminate most grievances over the setting of individual incentive rates. In both cases, the source of friction was removed principally because the union had a measure of participation in the rate-setting process.[4]

[3] Mary Barnett Gilson, *What's Past Is Prologue* (New York: Harper & Bros., 1940), p. 80.

[4] Frederick H. Harbison and John R. Coleman, "Working Harmony in 18 Companies," Case Study No. 13 in the series, *Causes of Industrial Peace under Collective Bargaining*, issued by the National Planning Association, Washington, D.C.

Fortunately, as we have indicated in the introduction, the question of whether or not the setting of a production standard is a matter for collective bargaining is now fairly well settled through a number of decisions of the National Labor Relations Board. Many of these decisions arose in connection with disputes over production standards under wage incentive payment plans. Section 8 of the National Labor Relations Act provides: "It shall be an unfair labor practice for an employer to refuse to bargain collectively with the representatives of his employees. . . ."[5]

The N. L. R. B. and the courts have supported the position under this clause that wage information which is necessary to the union for intelligent bargaining on the issues raised in negotiations must be supplied to the workers even when the contract is silent on the subject.

The latest case in which the union's role in the setting of production standards is once again confirmed is the Otis Elevator Company *versus* The International Union of Electrical Engineers. Significantly, however, in the Otis Elevator Company case the firm, in handling a grievance, refused to furnish or allow copies to be made of its file on the production standard for one of its operations. The union's request to bring in its own time study man to conduct a study of the job was also turned down. In both instances, the employer was found to have violated Section 8 (a), No. 5. The Court of Appeals limited this decision. It overruled one-half of the Board's decision. The court stated that the employer is under no obligation to open his plant to union representatives to enable them to make studies of their own. The final disposition of the case remains with the United States Supreme Court at this time.

These rights have imposed new tasks upon labor, and, accordingly, labor is now faced with the problem of developing technicians and techniques to cope with these new problems.

Carroll recommended that representatives of the workers take part in the actual rate setting but that they be regular mem-

[5] *National Labor Relations Act,* Section 8(a), No. 5.

bers of the plant standards department and thoroughly trained for the work.[6] It is this training of which the labor movement has been exceedingly suspicious, since it has in the past, at least, generally been as much a course of indoctrination in the assumptions of the particular time study organization as anything else. The trainees seldom have the academic background to distinguish between the assumption and the demonstrated fact. Generally this results in the union time study man's becoming a hostage of the management. Whenever a rate is in dispute, the union time study man goes through the same set of clerical operations as does the management man, and so, of course, comes out with the same answer. Since very few managements or workers' groups are so clumsily dishonest as to attempt to fake readings, practically every rate dispute (at least in my experience) reduces itself into differences on how data should be analyzed. Yet it is this very difference in interpretation and analysis which the union's management-trained time study man is likely to be unequipped to recognize and present.

Another consideration is the payment by the firm of the union time study man. The hopeless situation that this practice creates was perhaps best illustrated by the experience of the International Ladies' Garment Workers' Union in the Cleveland market from 1920 to 1931. The time study men considered themselves the employees of those managements which hired them. Their salary increases and their promotions depended upon the employer and of necessity their loyalty belonged to him.[7]

It is true that time study technicians are striving for the same professional recognition that, for example, medicine and accountancy enjoy, and thus might resent this statement as a slur. However, if an accountant were paid by his employer's competitor and a medical man by the patient's undertaker, it is

[6] Phil Carroll, Jr., "One View of Labor's Participation in Motion and Time Study," *Advanced Management*, Vol. VI, No. 2, April 1941.

[7] An excellent discussion of this problem is found in: Sumner Slichter, *Union Policies and Industrial Management* (Washington, D.C.: Brookings Institution, 1941), pp. 393–436.

doubtful that this recognition of professional integrity would have developed. It is much sounder for the union's time study technician to be responsible to the union and to no one else. At least, in my experience this has proved by far the most satisfactory arrangement.

A number of unions have created separate staffs to handle production standards problems as they arise. The International Ladies' Garment Workers' Union was the first labor organization to create a separate Management Engineering Department for this purpose. The United Auto Workers, C. I. O., and the Textile Workers' Union of America, C. I. O., soon followed. Other organizations like the United Rubber Workers, C. I. O., and the International Association of Machinists, A. F. of L., retain specialists in these techniques on their national and regional staffs. Smaller unions have called upon the specialists on the staff of the larger unions for special consultation.

The International Ladies' Garment Workers' Union, A. F. of L., has detailed the functions of its engineering department in a short statement that follows:

The Management Engineering Department of the International Ladies' Garment Workers' Union was created by the General Executive Board, July 22, 1941. Its objectives were listed as follows:

1. To assist in improving the manufacturing techniques and operating methods of all branches of the ladies' garment industry with which the earnings of our workers are intimately bound up. This will be done through plant inspections by department representatives, followed by specific recommendations.

2. To serve as a central information agency:

(a) for the determination of the level of "fair piece rates."

(b) to record the production system and manufacturing techniques under which these rates are paid.

(c) to assist in training shop members and committees in distinguishing bad time study practice from good time study practice, in the determination of rates.

Thus the Department serves both a policing and a cooperative function. However, the two purposes overlap and are not in distinctly different categories. Practically the entire women's clothing industry pays its wages on a productivity basis. That is, workers are compensated in accordance with their relative productivity. The plans vary widely but the predominant plan is ordinary piece work.

The function of the Department is to serve as a consultant to the officers

charged with the negotiation and administration of the actual collective agreements. During the negotiating sessions advice is given to line officers about the effect of the design of the wage incentive payment plan in use in the factory on the earnings of the workers. In addition, the production standards underlying the wage incentive payment plan are evaluated.

During the administration of the agreement the Department is consulted when new rates are set on new items scheduled for manufacture.

The technical services of the Textile Workers Union of America, along with the qualifications of the personnel that are required for its successful operation, have been detailed by Solomon Barkin, Director of the Department.

In response to general developments, and to those peculiar to the textile industry, the writer organized a technical service in the Research Department of the Textile Workers Union of America. A specialized staff was, after some years, recruited to aid him in extending the service, and it now consists of a senior and junior engineer in the national office and two field engineers.

Before describing the goals and the functions of this group, two conclusions reached early with regard to the organization of the service should be noted. First, the staff must consist of permanent personnel and not of consultants. There are practically no engineering consultants who could maintain an undivided loyalty to the trade union. They have been trained in management's approaches and techniques and depend upon management for their work. Few are prepared to serve trade unions without considerable re-education. They have no initial preparation for helping workers to protect themselves against management's authority. They are neither steeped in nor have had access to a comprehensive statement of labour's attitude towards these problems. To gain the necessary insight and understanding, and to learn how to adapt their professional engineering skills and knowledge to serve the labour movement, they must be given careful direction and enjoy an intimate and permanent relation with the trade unions.

The primary function of the technical service of the Textile Workers Union is to aid and train the Union's administrative personnel in the handling of problems of an industrial engineering character. The ultimate aim is to provide such training by demonstration and instruction as will make these officials self-reliant. They will then be able to understand fully the employers' proposals and their implications and will be acquainted with the methods by which they have been derived. After identifying the appropriate Union policy, which is seldom available in precise written form, they must be able to apply it in such a manner that it takes into account the local bargaining relationships and the long-term union and worker interests.

The above goal is constantly held to the fore. It means that the technical staff must remain small and place its emphasis on training. It also reflects the fact that, no matter how rational the argument and how complete and overwhelming the data, the final result in the vast number of cases will depend upon the balance of bargaining power. The Union's administrative personnel represent and direct its bargaining force and must therefore be the spokesmen in such negotiations.[8]

The United Automobile Workers Union, C. I. O., has detailed the methods of operation of its own engineering department in a time study manual written by the U. A. W.

The functions of the Engineering Division are as follows:

1. To provide local unions, through their Regional Office, with technical assistance in disputes about production standards, incentive rates, incentive plans and job evaluation plans.

The Engineering Division, of necessity, has a small staff. However, this staff will be most effective and its services will be made available as far as possible if the procedure outlined below is carefully followed:

2. When a local union has a problem concerning time study, incentive plans or rates, job evaluation, etc., the services of the Engineering Division of the Research and Engineering Department can be obtained provided that the local union has made no commitment to the company regarding the procedure which the representative of the Research and Engineering Department will use.[9]

The approach of the trade unions to the actual operating methods which they use in settling production standards disputes within a time study context can be studied from statements of their procedures and from specific cases which have arisen.

The United Auto Workers instructs its locals along the following lines:

HOW TO HANDLE A PRODUCTION STANDARD DISPUTE

For the reasons spelled out earlier in this manual, the U.A.W.-C.I.O. rejects time study as a faulty technique yielding unreliable results when applied by honest practitioners and subject to manipulation against the workers' interests when applied by the biased and the unscrupulous.

We do not deny to management the right to use time study if it so chooses, any more than we would permit management to deny us the right

[8] Solomon Barkin, "The Technical Engineering Service of an American Trade Union," *International Labor Review*, Vol. LXI, No. 6, June 1950, pp. 11–14.

[9] "U.A.W.: C.I.O. Looks at Time Study," pp. 82–83 from the advance manuscript of a proposed pamphlet.

to use methods of our own choosing to achieve our purposes. We are concerned with the end results of time study—production standards and piece rates. In fulfillment of our responsibility as a Union to defend the members against speed up, we reserve the right to challenge any production standard or piece rate regardless of the method used by management to determine it.

The worker's first line of defense against unreasonable production standards is his union representative in the shop—the steward, a committeeman or an officer of the local union. Any grievance regarding production standards should start by having the appropriate representative of the union present it to the company. However, the union representative must start the grievance out on the right road. What he does in the first stage of a production standard dispute will determine to a great extent the effectiveness with which the union can help him to defend the workers he represents. The International Union, therefore, urges that representatives of the union in the shop should be thoroughly prepared when handling production standard disputes, and recommends the following steps as a general guide for stewards, committeemen and local union officers.

1. *Remember that the real issue in dispute is not the time study, but the production standard. The time study only represents management's attempt to justify its production standard. Some time study man may have distorted and obscured the basic facts about how long it takes the worker to do a job by intruding his own personal judgments and a lot of complicated arithmetic. But the final result he produces is still just management's opinion. Since, as we have seen, there is no scientific and objective measure available, the answer to management's opinion must be the Union's opinion based on the knowledge and experience of the worker who actually has to do the job plus the informed judgment of his Union representatives. Wherever possible, try to settle the dispute on the basis of your best judgment of what is a reasonable production standard, without getting tied up in arguments about time study.*

2. Remember that a grievance regarding an unreasonable production standard must be begun in the same manner as any other grievance. A full investigation must be made, and all of the information about the disputed job must be gathered. Two umpire decisions under the General Motors Agreement (E-62 of 1947 and F-53 of 1949) have established the right of the union representative to demand from management, even where the agreement language is far from specific, all of the facts in management's possession bearing upon the dispute. After all of the available information has been obtained, the representatives of the local union should be prepared to bargain on the matter in dispute with the proper representatives of the company. There are a number of traps to watch for in bargaining on a production standard dispute:

(a) The company may argue that the dispute is not a bargainable matter, and may therefore propose that the local union get someone to time study the job so that a comparison can be made between a time

study made by the union and the one made by the company. NO LOCAL UNION SHOULD AGREE TO ANY PROCEDURE OF THIS KIND. It means acceptance of the principle of production standards based on time study.

(b) The company may propose that a joint time study be made, where a representative of the union and a representative of the company both time study the disputed job at the same time. A comparison is then made between these two time studies.

NO LOCAL UNION SHOULD EVER AGREE TO ANY SUCH PROPOSAL. This also implies acceptance of time study as a proper method of setting production standards. The Research and Engineering Department is willing and ready to help every local union protect its members against unreasonably high production standards or unreasonably low incentive rates. But its ability to help will be severely limited if you commit it in advance to following any particular method as inaccurate and unreliable as time study. Leave the representative of the International Union free to use whatever approach to your problem seems best based on his wide experience with similar situations.

(c) The company may propose that a third party be asked to time study the job, his determination to be accepted by both the company and the union. NO LOCAL UNION SHOULD AGREE TO ANY PROCEDURE OF THIS KIND. Again, it involves accepting the principle of setting production standards through time study. The time study yardstick does not become more accurate because a third party uses it instead of management. Besides, the third parties available for this purpose are, almost without exception, management consultants whose judgment may be warped by their financial dependence on management.

(d) The company may propose that a representative of the International Union's Research and Engineering Department be asked to make a time study and set the standard thus committing the International Union in advance to the method to be followed. NO LOCAL UNION SHOULD EVER AGREE TO SUCH A PROCEDURE. This also implies acceptance of the principle that time study is a proper method of setting production standards. The International Union's engineers are at least as competent as any others in making time studies. But competence in applying a faulty technique does not make the technique itself any better. The defects inherent in time study mean that results reached by a Union time study man, while free from pro-management prejudice, are subject to the same errors as a time study made by anyone else. (This does not mean that the International Union's representatives will never make a time study. But when they do time study a job they do so with full knowledge of the weaknesses of their results and only when they are convinced that it is an unavoidable step in the direction of resolving the dispute in the workers' best interests. When a U.A.W.-C.I.O. engineer makes a time study he does it as a frank and open compromise with principle dictated by the urgencies of a practical situation. It would be irresponsible, for example, to refuse to make a time study where refusal to make it would lead to a

strike while making it would smooth the path toward an acceptable settlement of the dispute on a peaceful basis.) THE JUDGMENT AS TO WHETHER OR NOT TO MAKE A TIME STUDY, HOWEVER, MUST BE LEFT TO THE INTERNATIONAL UNION'S ENGINEER BASED ON HIS EXPERIENCE IN HANDLING SIMILAR DISPUTES.

(e) One further trap sometimes set by management might be mentioned here even though it does not relate directly to bargaining on a specific dispute. Management will sometimes offer to have its time study man "train" the union's time study stewards so that they will "understand" the procedure he uses. Sometimes management will offer to send the stewards to school to study time study at management's expense. *NO LOCAL UNION SHOULD EVER AGREE TO SUCH PROPOSALS.* The only schools available for this purpose are management oriented; and no management time study man will try to give a union representative an honest understanding of the unavoidable errors and arbitrary, personal judgments with which time study is filled. If he did so, he would be promptly fired. The International Union, through cooperation between the Education Department and the Research and Engineering Department, conducts frequent time study classes at union summer schools throughout the year. Local unions should see that officers who are faced with time study problems attend these classes. There they will learn the truth about time study, and the proper methods to be used in protecting their members against it.

All of the above management proposals are designed to avoid collective bargaining on production standard disputes, and force the local union to accept and use time study as a means of determining production standards. NO LOCAL UNION SHOULD ALLOW ITSELF TO BE PUT IN THIS POSITION. Here are the reasons why. It is management which says that it can establish reasonable production standards by time study. The union does not accept this claim, and therefore cannot be a party to resolving production standard disputes by using time study. Moreover, the fact that a company says that it or anyone else can set reasonable production standards by time study, does not make it so. The union must in all cases demand that the company present unquestionable evidence across the bargaining table to prove that it can in reality do those things that it says it can do.

This means that, where it is not possible to resolve a dispute without getting involved in time study, the union should require the company to submit its time study to a complete and searching analysis, and to bargain collectively on the basis of all of the weaknesses shown in that time study, and all of the other facts of the dispute. The inaccuracies of time study, which have already been pointed out, indicate such a large margin of error as to make it completely unreliable as a means of resolving production standard disputes. *In almost every case the difference between the company and the union in a dispute is less than the margin of error involved in a time study.*

3. If, despite your best efforts, you have been unable to resolve the dispute without getting into the time study, it will become necessary to subject the time study to thorough and searching examination. Start right at the beginning. Carefully examine the company's detailed record of the job, taken from the time study man's work sheet. If the record does not contain sufficient information so that the job can be reproduced exactly as it was when the time study was made in terms of both physical set-up of the job and the general working conditions, there is no way of checking whether the conditions on the job now are the same as they were when the time study was made. If that is the case, the company should be told that for that reason alone its time study is completely useless.

4. If the time study man's detailed record of the job contains any information on the conditions prevailing when the time study was taken, compare that record, item for item, with the methods and conditions under which the job is being done at the time of the dispute. Find out whether the job is being done in the same way as it was when the time study was made. Here are a few reminders of items to be checked:

Is the material the same?

Is the material placed in the same location?

Is the material brought to the job in the same way?

Is the material removed from the job in the same way?

Is the machine operating in exactly the same way?

Are the same tools and fixtures being used?

Is the motion pattern of the worker the same?

Do the workers now doing the job have the same amount of training and experience as the worker who was time studied?

Note carefully any differences between the details of the job as it was set up for the time study, and as it is being performed at the time of the dispute. These differences may require more time for the job. If so, they should be used in bargaining with the company to get more time.

Remember: One of the frequent inaccuracies of a time study arises when a job is not operating under exactly the same conditions and circumstances as those in effect when the time study was made. In addition it may be that the time study man's record of the job in dispute is incomplete, or inaccurate, in which case there is no way to make the above check. However, an incomplete or inaccurate record would nullify the company's case, and support the union's position in opposition to the disputed production standard.

5. Read over the time study man's record of all the work elements very carefully to be sure that each one follows logically from the next one, and that the beginning and ending of each work element have been carefully defined. When the beginning and ending of each work element are not clearly defined, any attempt to establish a time value for its performance is nothing more than guesswork.

6. Check the time study man's description of each work element against the work which the worker on the job is actually performing. Make sure that no work has been left out. When work is being performed which is

not recorded in the element breakdown, then such work should either be discontinued, or it should be included in the element breakdown and additional time allowed for it.

7. Check the time readings recorded by the time study man to be certain that the time study was made by continuous timing. This can be done by starting with the watch reading recorded for the first work element, and following each recorded time to be sure that each time value follows logically from the previous one.

Remember: If the time study was made by snap-back timing, there is no way of checking to make sure that the time study includes all the time that was acually spent by the worker in performing his job. You can be certain, however, that with snap-back timing the time study man was unable to record the time that was actually spent resetting his watch at the conclusion of each work element, so that the job will require more time than his time study record shows.

8. If the study was made by continuous timing, be absolutely certain to check the arithmetic by which the time study man figured the time required for each work element from the successive watch readings.

9. Find out exactly how long the time study lasted, and at what time of day it was made. Then ask yourself these questions:

Was the time long enough to include everything that will face the worker in the performance of his job?

Was the time study a reliable sample of the day's work, or was it made during a part of the day when the worker was fresh and apt to be working faster than he would later on?

The company should be required to prove by standard sample-testing procedures and common sense that the sample—or what is left of it after "strike-outs"—is a proper one.

10. Find out what rating factor the time study man applied to the time actually taken by the worker he studied. Check to find out when the time study man recorded his "rating factor." There is a possibility it was juggled to produce a predetermined result if it was not recorded and shown to the worker being time studied and to his steward before the time study man left the job. Next compare the rating factor with what the worker believes is a proper pace. This can be done by watching the worker who was timed while he works at what he considers a proper work pace. Use your own best judgment to decide whether or not he is working at a reasonable pace. Then compare the amount he can produce in a given time with the company's production standard for the same length of time taking into account the fact that something must be added to actual time for allowances. For this purpose the clock on the wall will do. You do not need a stop watch because, as we have seen, nothing of real value is gained by breaking the job down into parts. In this manner you can judge for yourself about how far off the company's time study man actually is. Your judgment with regard to work pace is just as valid as that of the time study man. As in other questions that come up about time study, the combined judgment of everyone involved in a dispute will

produce a much more equitable result than the judgment of the company's time study man alone.

11. Find out whether the time study man used all the element time values he measured. If he discarded any, make sure there is a clear explanation on the record of why they were not used. If he struck out any time values just because he considered them to be "abnormal," or by applying a mathematical formula, it is certain that the "representative times" he finally decided upon are nothing more than a guess. If specific reasons are given for striking out any values, decide whether the reasons are valid. Check with the worker on the reasons given by the time study man for his "strike-outs." Make sure the situations for which times were struck out do not include those that would recur on the job. Remember that minor variations in the motions used, or incidents such as a fumble or dropping a piece, are not valid reasons for striking out a time, because they do happen from time to time on any job. The worker is a human being, not a machine.

12. Check the "representative" element time values to be sure that only a simple average is used in calculating them.

13. Check to be sure that the percentage figure that is used to calculate the time for allowances for personal time and fatigue gives the worker the time to which he is entitled.

14. Check to find out how many of the pieces produced during the time study passed inspection and how many were rejected. If there is no record, there is no way of determining whether the pieces produced during the time study met the company's quality standards. The company's time study then becomes worthless because it is obviously improper to compare the time required to produce good pieces with the time taken to produce scrap. If there is a record, compare it with the proportion of scrap or rejects produced by the worker at the pace he considers normal. It may be that although he is producing a smaller *total* number of pieces per hour, his slower pace enables him to produce more *good* pieces acceptable by the company's standards. It is also important in this connection to make sure that there has been no raising of the quality standards applied by the company since the time study was taken. If quality standards were raised or inspection tightened after the time study was taken, the number of acceptable pieces that can be produced per hour will be reduced and the amount of scrap increased.

15. Make a final check of the time study man's arithmetic in all calculations.

If a grievance cannot be settled without getting into details of the time study, the union representative who carefully checks all the above points will be in a good position to defend the worker in bargaining with the company. Most disputes can be satisfactorily settled if the errors and evidence of bad judgment—and often bad faith—on the part of the time study man are clearly presented in the collective bargaining sessions with the company. If the company still will not agree to a settlement, a union

representative may wish to make an independent check of the job to get more information. For example, the union might find it useful to establish the amount of time actually lost in a day due to delays on the job. If a satisfactory settlement cannot be reached through collective bargaining by the local union officers, the Regional office should be called on for help. The Regional office will do what it can to settle the dispute and, if unable to, will decide whether to ask for help from the International Union.

The basic thing to remember at all times is to maintain faith in your own best judgment. In the last analysis, judgment is the basic and decisive element in the time study man's conclusions. Your judgment has just as much value as his.[10]

These recommendations reflect the historical experience of the automobile workers when attempts were made to take advantage of workers' ignorance of the technique. The reason that management is on the receiving end of its own boomerang is implicit in some of its past practices.

The files of the War Labor Board are replete with case after case in which unions have attempted either to throw out a wage incentive payment plan or to impose such restrictions upon the methods of developing production standards as to make operation of the plan all but impossible. A good example of what is meant is illustrated from a case involving a Detroit auto parts company and the United Auto Workers Union in which I acted as consultant to the union. The details follow:

. . . the immediate grievance that brought the whole time study procedure into question was a disagreement that arose as the result of a faulty rate. The particular task at issue involved a lathecutting operation of specified dimensions on a piece of normalized steel.

The company considered the rate set on the job too high. Specifications were changed to call for the use of heat-treated steel, which was much harder than the normalized steel previously used, thus cutting down the machine speed and feed. The correct method of revising the rate would have been to isolate the cutting time element from the previous study and revise the allowed cutting time upward to suit the new speed and feed. The company obviously used this change in operating method as an excuse to make a new time study, which, through rerating the operators, resulted in a production rate higher for the hardened steel than for the normalized steel. It should be explained that in general relationships be-

[10] From a forthcoming publication on time study of the United Auto Workers, C.I.O.

tween this company and the union had never been cordial. Accordingly, the union demanded that detailed time study procedures be written into the agreement and that certain engineering standards be prescribed in the contract; the extracts quoted below also give the reasons for each provision insisted on by the union.

Submission of a description of the instrument used because experiments have indicated a wide source of error which may be traced to this source. . . . The length of the elemental readings taken likewise influences the accuracy of time study. The accuracy of elemental readings varies inversely as the elemental time lengthens. The number of readings taken is important to determine whether or not the statistical laws of sampling have been followed. Different authorities recommend different numbers of readings for different reasons, all of which, unfortunately, yield different results in the standard.

The particular statistic derived from the array of data is important because here again there is no widespread acceptance of any particular statistic. . . . Other authorities advocate use of the mode, still others favor the median and still others the arithmetic mean. In other words, the same array of data will yield different results, depending upon the use of the particular statistic derived from the data.

The basic idea behind the assignment of contingency time allowances is necessary to determine whether or not the standards department has adhered to sound statistical practice. Likewise, the allowances for fatigue and personal needs have created constant sources of friction between the engineers and the industrial psychologists for many years.

More important than any of the preceding sources of error is the source of error found in the method of leveling, the device which is used to reduce observed data to data of the so-called "average individual."

It is for these reasons that the Union is calling upon the National War Labor Board to direct that Union time study observers jointly level time studies with members of the company's standards department. . . . We feel that the pooling of both judgments before the rate has become a grievance matter and will do much to nip rate disputes in the bud.[11]

It should be quite obvious that the weakness of the time study procedure was being used in this case by the union in an attempt to throw out the whole wage incentive payment plan because it found it impossible to get along with the company's engineers. Union demands were made more to hamstring the engineers than to seek a solution.

The nature of management's right to set production standards was spelled out in the decision of the arbitration board set up to

[11] William Gomberg, "Union Interest in Engineering Techniques," *Harvard Business Review*, Vol. XXIV, Spring 1946, pp. 362–363.

terminate the strike of the employees of the Ford Motor Company over an alleged "speed-up."

> . . . The "right of the company" [to establish and determine and maintain and enforce standards of production] which is "fully recognized" is not a right to make a final and binding determination. It is not like other "rights" specified in Article IV, as for example, the right to "decide the number and location of plants" or the "products to be manufactured" or the "schedules of production" or the "starting and quitting time." As to these matters, the company may make *final* determinations which the union must accept for the term of the contract and which may not be made the basis of strike action during that term. Such is not the case with respect to production standards. There the right "to establish and determine and to maintain and enforce" is more in the nature of a *right to initiate*. . . .[12]

The panel then turning to the specific issue before it, ruled that:

> Under contract which gives the company the right to maintain and enforce standards of production, the employer may operate its production lines at a speed in excess of the desired production schedule. However, it must seek to make each employee's work assignment as measured by standard work minutes equal to or within actual production cycle time available to him. In cases where the speed of the production line results in requiring employees to regularly work at a speed above standard, the *parties should work out a solution to fit the situation.*[13]

In other words they must bargain out a mutually satisfactory solution.

Attempts to develop machinery for the satisfactory development of production standards mutually acceptable to labor and management have led to two schools of thought. The first maintains that the initial setting of the production standard shall be a management function. Dissatisfied workers are permitted to register their dissatisfaction with any production standard through the normal grievance procedure. The second maintains that the setting of production standards should be a joint function of management and labor operating through joint administrative bodies.

[12] B. Selekman, S. Selekman, and S. Fuller, *Problems in Labor Relations* (New York: McGraw-Hill Book Co., 1950), pp. 338–339.

[13] *Ford Motor Company,* 12 LA 949, July 1949.

The United Automobile Workers Union, C. I. O., indicates that it prefers not to participate in the actual rate-setting function but always wants to be free to protest the rate that is actually set.

The U. A. W. has carried this policy to great lengths. Originally, the union was excluded from the setting of production standards because management insisted that this was its unilateral prerogative. Under the circumstances the union reserved the right to strike over production standards. For example, under the Ford agreement the impartial umpire arbitrating disputes under the agreement is explicity excluded from the area of production standards. This was the pattern for the industry.

The union grew to find this freedom of action so attractive that it endured a long drawn out strike against the International Harvester Corporation. The Corporation wanted to eliminate the union's right to strike over production standards but was willing to make the settlement of production standards disputes arbitrable.

The final compromise was Article 12 of the International Harvester-U. A. W. agreement. This clause gives the union the choice between taking the production standard dispute to arbitration or of using an alternative procedure which follows: First, provision is made for joint study of the job by representatives of the management and the union. If the union is still dissatisfied, the local union is free to take a strike vote.

The International Ladies' Garment Workers' Union has used both the administrative and grievance methods of vote settlement. Where a large concentration of workers in one community like New York City has warranted the additional expense to the union of the joint administrative method it has preferred this way of approaching the problem. On the other hand in smaller communities where the small revenue from the membership cannot support the expenses of this method, it has operated successfully under the first method, which confines union consideration of rate-setting problems to the grievance procedure.

The International Ladies' Garment Workers' Union technicians have found that much more important than the machinery used are the technical criteria used to judge the fairness of the rate set by the parties, under either type of administrative arrangement.

Attempts to foist an obsolete set of time study system assumptions upon a joint administrative board charged with the setting of production rates under the second method will be as fruitless as the insistence of a company that its time study methods with all their limitations shall continue the sole criterion of whether or not a rate grievance is justified under the first, or grievance, method.

Where the International Ladies' Garment Workers' Union has made use of the joint method, such as in the New York Dress Industry, the foundation for joint operation has been a system of standard data which is in a constant state of revision and development. The acceptance of this standard data does not imply a belief in its technical infallibility but rather it provides a convenient basis for a contractual relationship.

Where the grievance method has been employed, the position taken by the union is that the employer is free to use whatever technique of time study he pleases in developing his offered rate. The union maintains its freedom, however, to subject that rate to the test of whatever time study techniques it feels best fit the situation. Under no circumstances does the union become a party to the particular set of assumptions making up the employer's time study technique. Such an approach, of course, decreases the importance of the particular administrative approach to the resolution of rate-setting disputes. In the course of investigating a grievance, all of the same technical problems come to the fore that require solution under the joint administrative method.

No rigid pattern of management prerogatives has been respected in this field. The attempt rather has been to develop relationships that fit the state of extant technical knowledge,

within a pattern of genuine rather than legalistic collective bargaining.

These empirical procedures are best illustrated by the following cases to which the I. L. G. W. U. Management Engineering Department was a party.

The first case occurred in one of the larger undergarment plants in New Jersey, where rates were very carefully set by the members of a competently trained time study organization. The lockstitch sewing machine operators were classed in the job evaluation at a 65-cent hourly base rate. The so-called special machine operators, whose work did not require so much skill as lockstitch work, were pegged at 60 cents an hour; while the floor girls, who performed much simpler hand operations, had a rate of 45 cents an hour. Production standards were then set to permit these workers to earn an incentive increment.

The extremeness of the results may well be called unexpected. Within six months after the rates were set, the lockstitch sewing machine operators were earning only 73 cents an hour, while the special sewing machine stitchers had risen to 90 cents an hour, and—most startling of all—the unskilled floor girls had gone up to $1.05.

Of course the superficial answer is to say that the standards were not set correctly in the first place. But the important question, which is seldom asked is this: *why* weren't the standards set correctly?

In judging whether or not rates are set adequately in any wage incentive payment plan, the fundamental approach of the union rests upon the established principle that a given group of workers must be afforded an earning opportunity commensurate with the average hourly earning capacity they have already demonstrated. This, with some modifications in language, has been the doctrine of the War Labor Board also. There is no need to quibble whether this principle, in this specific case, enabled either the I.L.G.W.U. or the W.L.B. to solve the problem completely. It did not. Imperfect as the principle is, however, it provides the only usable criterion.

But to go on with the story: The company attempted to circumvent its problem by merely redesigning the method used by the floor girls and the special sewing machine operators whose earnings were abnormally high, in order to bring their rates into line with the basic job evaluation plan. The workers involved naturally viewed this step as a rate cut, which is exactly what it was for them, since the firm was devising a new method as an excuse to rerate time studies.

The union, for its part, fully understood the management's problem. It was clear that the company was not so much concerned with saving a few cents in direct labor cost as it was with adjusting the inequities developing in its basic rate structure. Certainly this was cause for alarm. Since the social hierarchy within a plant is linked to the relative wage payments received by each craft, these inequities were creating havoc.

When the "aristocratic" operators found themselves earning so much less than the "menial" floor girls, confusion and unbalance were inevitable.

The union managed to settle the matter, through what was admittedly a makeshift solution, by computing the direct labor savings afforded the company by the change in rates and by proposing that each worker be given a lump sum of one year's direct labor saving per worker. The firm agreed to this, though for a somewhat shorter period than the union had suggested.

The important thing to remember in this connection, however, is the fact that the workers soon grasped the secret that the fundamental purpose of the wage incentive payment plan was not to encourage labor's unrestrained productivity but to keep the engineer's plan in balance! For the record shows that the workers adjusted their future production to match the wage design that the company wanted, and six months later the firm's time study department was proudly displaying evidence to the I.L.G.W.U. representatives that the rates were in balance. The rates were indeed in balance, but that fact symbolized a meagre triumph. The essential problem had been brought no nearer to a solution. The time study technique, which had failed to predict what actually happened under the rates set according to the first version of the incentive plan, still failed to demonstrate that it would determine the full production possibilities of these workers on future jobs. It cannot be emphasized too often that what we are confronting is a deep-seated technical weakness of the time study technique.

Difficulty of Determining Criteria. The second example likewise deals with an underwear factory in New Jersey. This plant had previously set rates by what it claimed was a careful time study technique based upon standard data. The production control standards, moreover, were highly unstandardized. Among other instances of this looseness was the fact that workers were sometimes compelled to work on as many as three or four styles per day.

As in the first case cited, the union's sole criterion of whether or not a rate was adequate under these circumstances had been the earning opportunities afforded an individual worker to make his or her average hourly earning rate already demonstrated. Because of the management's lack of production control, however, the union's effort in this plant had been reduced in most cases to a straight program of bargaining, thinly coated with a veneer of time study terminology.

Later, the firm on its own initiative was able to redesign its production control system. To some extent this forward step was due to the war, which permitted the management to cut down substantially the number of styles in process. The result was that workers were kept on the same style continuously for as long as sixteen months. The first production lines set up under this new one-style system indicated that worker's earnings continued to increase over a period of four months; after that they leveled off.

The problem, then, was one of establishing proper rates under the new

system. The solution proposed by the union is indicated in a letter written by the I.L.G.W.U. management engineering department. The pertinent portions of this letter are as follows:

"In reference to the first question that you raised: What protection does the company have that piece rates will be set so that wages will not spiral indefinitely? The company claims that this has been the result in the past. Rates presumably set to yield a fixed average hourly rate actually yield a much higher rate when workers are kept on the same operation and the same style for a substantial period of time. The rate set for a new style has been set on this newly demonstrated average hourly rate and, of course, a few months later this rate likewise results in a new increase in the average hourly rate thus continuing an indefinite spiral."

The clause proposed to cover this situation follows:

"Among the variables which shall be considered in determining the piece rate for sewing operations shall be the expected length of continuous time that the particular style will be on the sewing floor and sewn by the same operators without interruptions by other styles.

"The second question that you raised was: What shall the average hourly rate be during the time that the worker is on time work learning the new operation so that she will be capable of producing at maximum speed when shifted from an old style to a new one? I would suggest a learning period of time work for at least six weeks. During this period, operators should be paid 90% rather than your offer of 85%, of his or her demonstrated average hourly earning capacity with the new system, provided that this is the same or exceeds the average hourly rate under the old system. If the hourly rate under the old system exceeds 90% of the hourly earning rate under the new system, then the former rate should be paid to the worker.

"I believe that such a system would enable workers to maintain their hourly earning level and at the same time be subject to an incentive to reach top production. Naturally, the direct labor cost during the 'retooling period' while new styles are being introduced will be higher than during the regular period. This is a legitimate management expense which should not be transferred to the workers. On the other hand, the company receives what it is entitled to—the incentive to reach top production."[14]

A study of these cases will indicate that no attempt is made to detail an elaborate time study procedure. Rather an attempt is made to determine loosely the criterion to be followed in administering the agreement. In another case in which the union and the firm's engineers were unable to come to any agreement, both the company and the union engineering department were in the process of determining where their timing practices differ.

[14] Gomberg, *op. cit.*, pp. 360–362.

A large number of complaints on rates led to a proposal that both the union and plant engineer take a number of simultaneous time studies in order to determine what the principal sources of differences were. The firm insisted upon the use of a table of standard data which the union found in the plants after the collective agreement was signed. The union refused to be bound by the standard data inasmuch as it was not a party to its formulation. Table IX indicates the comparison of standard time units assigned from the firm's library of standard data with the standard units that the company engineer derived from his rated time studies and the rated time unit assignments made by the union's engineer from the same time studies. In addition, a comparison of the actual rating of the two observers is indicated. The union argument in indicating that its figures were more nearly correct than those of the company was based on the fact that from past records of time studies, amounting to 42.55 hours, the weighted mean rating was 84.8 per cent. Inasmuch as the factory employed 300 people, the union maintained that, since 100 per cent indicates a normal worker, this was prima facie evidence of tight rating. These figures and methods of procedure are not given to make out a case, but to indicate the empirical methods that are in the course of development. None of these calculations will settle anything finally; they are instruments in a give and take process that will in all likelihood come to a successful outcome because of the cordial relationship existing between the company and the union.

Fortunately, the union shop has come to be taken for granted in many of the basic industries. Sooner or later the rest of American industry will catch up with its natural leaders.

On the other hand, Slichter concludes from the failure of the Naumkeag experiment in joint administration of work-load standards that "co-operation with Management to increase job assignments is not impossible for a union, but that it is likely to produce revolt, particularly at the very time when co-operation is most needed, namely during periods of depression."[15] This is

[15] Slichter, *op. cit.*, p. 559.

TABLE IX

Studies Taken Simultaneously by Management and Union Engineers at Company X

1	2	3	4	5	6	7	8	9	10	11	12	13
6282	Blind Stitch Bottom	.188	.501	.504	.146	.140	120	111	−22.4	−25.6	4.12	− 7.5
6282	Blind Stitch Bottom	.171	.503	.501	.140	.134	115	111	−18.1	−21.7	− 4.28	− 3.48
6310	Piece Back Skirt Panels	.188	.742	.733	.201	.183	108	100	+ 6.94	− 2.7	− 8.96	− 7.4
6285	Hem Sleeves Single Needle	.177	.636	.649	.202	.177	125	108	+14.1	0.0	−12.4	−13.6
6308	Set Sleeves	.268	1.265	1.243	.382	.312	120	100	+42.5	+16.4	−18.3	−16.7
6308	Pink Sideseam & Close Sleeves	.464	1.582	1.594	.554	.477	120	111	+19.4	+28.1	−13.9	− 7.5
6305	Pink Left Sideseam & Close Sleeve	.461	1.264	1.290	.425	.397	125	114	− 7.80	−13.9	− 6.6	− 8.8

Col. 1—Style Number
Col. 2—Operation
Col. 3—Units Assigned by Firm's Library of Standard Data
Col. 4—Actual Cycle Time Recorded by Union Engineer
Col. 5—Actual Cycle Time Recorded by Firm Engineer
Col. 6—Standard Units Assigned by Union Engineer after Leveling
Col. 7—Standard Units Assigned by Firm Engineer after Leveling
Col. 8—Rating Factor by Union Engineer Assuming 100% normal
Col. 9—Rating Factor by Firm Engineer Assuming 100% normal
Col. 10—Percentage Difference between Standard Data & Union Eng.'s Unit Assignment
Col. 11—Percentage Difference between Standard Data & Firm Eng.'s Unit Assignment
Col. 12—Percentage Difference between Firm's Engineer & Union Engineer
Col. 13—Percentage Difference between Rating Factor of Firm and Union's Rating Factor

not so much an argument against joint administration of rate setting as it is a demonstration that all labor relations tend to collapse during a period of economic panic such as we had in 1932. The odds are that the union would have been wrecked in any case if it had fought the increase in the work loads. Joint participation in the rate-setting procedure, if anything, staved off the disaster for some time.

Some exceedingly optimistic claims have been made by Ruttenberg and Golden. They write:

> Give workers as a formally organized group a say-so in setting wage rates, work standards, job evaluations and in making time and motion studies, and they will produce all they can within the limitations of physical endurance, health and fatigue.[16]

This conclusion follows from the authors' conviction that

> . . . a desire for self-expression is usually present in every individual in an industrial enterprise and consciously or unconsciously, each one constantly seeks some way to express himself, except perhaps, industrial workers who have been numbed by years of hard toil and repression.[17]

The chief claim made for worker participation in the setting of the production standard is that it leads to a more peaceful and productive relationship than there would be without it. It is doubtful that its effect is a total release of the motivating drive which leads to maximum production. This matter of self-expression in industry has been somewhat overemphasized, perhaps to compensate for its having been completely overlooked previously. I have expressed my opinion on this subject earlier as follows:

> Actually, there is ground for suspecting that the great majority of those people who require this so-called self-expression outlet eventually wind up as local union leaders or members of the management hierarchy. Most of the others succeed in expressing themselves adequately in other leisure time pursuits.
>
> It is true, of course, that many workers have joined unions to overcome frustrations arising from being the victims of management's every whim

[16] Clinton S. Golden and Harold J. Ruttenberg, *The Dynamics of Industrial Democracy* (New York: Harper & Bros., 1942), p. 250.
[17] *Ibid.*

and fancy. But once this right is established, the majority remain satisfied —so much so, it may be added, that there is often difficulty in securing adequate attendance at routine union meetings.[18]

Having established the necessity for joint participation in the rate-setting technique, and how much can be expected from it, the problem now becomes one of designing such social arrangements as will maximize the effective results of such a relationship. A thorough examination of this problem would constitute a study unto itself. Various aspects of it have been touched upon by Slichter, Murray and Cooke, and Van Dusen Kennedy.

It is too early to attempt to formulate any rules that are universally applicable with the possible exception of one, and that is the necessity for union security, if the relationship is to be effective. It should be emphasized that union security is a necessary condition for a good relationship; it is not a sufficient condition.

Golden and Ruttenberg have emphasized that a union must above all preserve its institutional existence. They observe: "A union is not free to adjust a member's rate [piece rate] downwards when as a consequence, he can immediately quit the union and agitate for other members to follow his example."[19]

Once this union security has been established and the union technicians find themselves operating within the present limits of the time study technique, resort is taken in purely empirical method on a live-and-let-live policy.

This relationship is the core of the success of the methods that are in use. Recently, in a four-way panel in which one of the topics of discussion was the use of industrial engineering techniques in collective bargaining, one of the conferees complained: "I think interpretation of the language of the agreement causes us more trouble than anything else."

My reply was:

In practically every one of our factories once the agreement is signed, it is locked away; and that is the last that is heard of it. It seems to me

[18] Gomberg, "Union Interest in Engineering Techniques," *op. cit.*, p. 365.
[19] Golden and Ruttenberg, *op. cit.*

that there is trouble over the meaning only when both sides have become shop lawyers trying to read into the agreement what is not there. And, of course, the agreement can't cover everything that will happen unless the negotiators are rivals of Nostradamus.[20]

Any attempt to write into an agreement a solution for every collective bargaining problem that will arise from the use of time study techniques for rate-setting purposes will certainly require a seer to rival this medieval prognosticator. Solutions must arise from a new concept of human relations—a phrase I use with some hesitancy because of its abuse by a group whose approach is that of salesmen selling "psychology."

In their nostalgic longing to regain the power that they exercised over all industrial functions, including the unilateral setting of production standards, some members of management have resorted to a searching of their inward souls. Many of them are confused by the fact that their industrial effectiveness must henceforth flow from their ability to persuade people to follow them rather than their power to compel people to do their bidding.

Like many people who are ill and resort to patent medicines, they have taken up the study of human relations without attempting a basic diagnosis of the problem. What is needed are new concepts of collective bargaining and the development of social designs to carry these concepts into actual operation. Instead of concentrating upon this basic question, many members of management now hope to be able to exercise their arbitrary wills with a smile and a slap on the back since the whip and the grouch were ineffective.

The result has been a profusion of manuals on human relations for executives. They are concerned with a method of handling people, not living with them.

A good example of what I mean comes from Eugene Wilson's introduction to Stowers' book on human relations. Mr. Wilson wistfully complains:

[20] *Constructive Employee Relations in Unionized and Non-Unionized Plants* (Personnel Series No. 99; New York: American Management Association, 1946), pp. 18–19.

For years many of our keenest minds have been directed along engineering lines. Applied science attracts men of logic because it deals with materials that can be expected to respond to the same forces in the same way at all times. The modern effort to apply these exact processes to the handling of people or, even worse, the tendency to neglect human relations altogether has contributed to a serious situation which now threatens to undermine our social structure. The very fact that management has been forced *to surrender to outside organizations* many of the authorities that are vital to the discharge of its responsibilities, is itself a serious indictment of modern industrial leadership. We need to rediscover spiritual values lest the product of our creative endeavors destroy us.[21]

At the present time, most of these problems, including rate setting, have to be settled on an empirical basis. This calls for techniques that find their most accurate expression from Chester Barnard, President of the New Jersey Telephone Company: "Successful administration has to be based on intuitions that are correct, notwithstanding doctrines that deny their correctness."[22]

It will be recalled from our theoretical analysis of time study techniques in Part One that the margin of error of a time study was in excess of the collective bargaining increment or decrement of five per cent which we had set as a workable criterion. Most of this error is indeterminate because of the variability in the chance cause system.

Our summary of Part Two indicated that the arbitrary techniques of most rate-setting practitioners in industry not only lack a rational base, but are deficient even in a sound empirical base.

The procedures which we have outlined in this chapter constitute attempts to live together industrially. Unfortunately, they cannot yet be reduced to any set of rules or principles. The examples cited indicate attempts to pursue lines of investigation that thus far yield inconclusive results. They represent efforts to formulate new social designs within which rational collective bargaining can take place. The formulation of any

[21] Harvey Stowers, *Management Can be Human* (New York: McGraw-Hill Book Co., 1946), p. viii. (Italics mine.)

[22] As quoted in: Mayo, *The Social Problems of an Industrial Civilization*, p. 34.

set of definite rules will have to await a wider accumulation of experience.

Meanwhile, however, "man must make a living"; women need clothes; men need automobiles; production must continue.

We have attempted to give a rational analysis of the present state of the rate-setting art. Ernest Jones, the biographer and colleague of Sigmund Freud, reports a revealing incident. Freud had just been hailed at a dinner as the discoverer of the unconscious and the father of psychoanalysis. Freud arose to reply. He disclaimed that he had discovered the unconscious. He pointed out that he had merely attempted to bring the scientific method to the intuitive insights that the poets, novelists, and other great literary figures of the past had revealed. Perhaps we can best keep our sense of balance by closing with a picture of the rate setting process as seen through the eyes of a novelist looking at a garment factory. The operator is talking to the time study supervisor of the Sleep Tite Pajama factory. Mr. Hines is the hapless time study man.

Trouble, why your wonderful Hines set a rate on this here elastic job so low that . . . himself couldn't make 90 cents an hour on them. He's a sneaky shifty rat, that's what I think of the great Mr. Hines and I told him so myself. Took me an hour and a quarter to get out my last bundle. . . . if it isn't a crime and a shame what I put up with around this dump.

The setting of rates and the reaction by the operators is a routine you have to go over again and again, forever and ever apparently, as long as there shall be garment rates, and broken needles. You study carefully and set the rate, say 49½ cents a dozen for hemming. You post the rate and then the operator has a fit. She says she can't "make nothing on that rate." She says she's been here on the line for 23 years and no young squirt time study man is going to push her around. You tell her there's a lot of factors involved (what would we do without those involved factors?), and that management wants to meet the operators half way and continue to enjoy mutual confidence for the highest production, Sleep Tite quality, and high earnings based on output and ability. The operator says she "can't buy no groceries on mutual confidence" and is going to go to work at the Packing Plant unless something is done about the rate. The other girls in the unit glare at management and exchange significant looks and talk so much about it all day that production goes off 12 dozen.

You promise to analyze the situation and make an "eight-hour study" to check for any factors that might have been overlooked (or involved).

Next time, the time-study man takes an all-day study and finds that he forgot to allow one-half minute per hour for "grasp scissors with left hand, clip estimated two threads per dozen, return and release scissors to position on machine table," and this raises the rate .0015 per dozen.

You announce this raise to the operator and she invokes the *Sherman Anti-Trust Law* and the *Emancipation Proclamation* and says she is being crucified.

Then there is a formal protest by the union president. You hold a meeting in his private office. "Mutual confidence" is mentioned.

The operator says she can't eat mutual confidence and that she can't make even 75 cents an hour on that rate, and she has been here on the line 23 years.

You agree to raise the rate from 49½ cents per dozen to 50¼ cents. Two weeks later the operator is running away with the rate and making $1.33 an hour, within 17 cents of the machinist, who is the best machinist in the area and knows more about a Singer machine than the Singer people.

Sometimes you refuse flatly to raise the rate, but it is understood, according to the unwritten rules of this peculiarly formalized game, that the fun is over now and the jabber is at an end. Whether there is a little adjustment made or not, after this conference the operator goes back to work and quits wasting time squawking. It's been a nice break in the monotony and the operator has played it for all it was worth and squeezed the part dry. Now let's get to work again![23]

Just what have we engineers to contribute that will improve on this very human process.

[23] Richard Bissell, *7½ Cents* (Boston: Little Brown & Company, 1953), pp. 132–134.

Appendix

The appendix which follows illustrates the detailed method of data testing to which reference was made in Chapter 4.

Kendall specifies a method for deriving sampling distributions in an exact form when the parent population is completely specified but not necessarily Gaussian. However, it leads generally to complicated integrals whose results have not been tabulated.[1] Unless the mathematical statisticians or some interested time study groups develop tables for this wide variety of integrals, these methods will offer little useful material of value for the time study technician.

When we realize, moreover, that we have no previous knowledge of a parent population, but must infer its characteristics from the sample, it becomes doubtful whether the expected results justify the effort to determine these values. Sometimes this evaluation problem can be circumvented by attempting to fit one of the curves of the Pearsonian system of curves to the parent sampling distribution whose integral remains untabulated. However, this still presupposes a knowledge of the exact form of the universe of data. Should we find upon investigation that all time study data under a condition of statistical control approximate ideal form or at least some hitherto undesignated shape which Wiberg describes, then these complex tools might become useful.[2]

Meanwhile, we must resort to methods of inferring sample statistics from a population when the shape of the universe is unknown. Quite fortunately, Tchebycheff has developed a method to set the limits within which any particular sample statistic will vary with a pre-assigned consistency.[3]

[1] Maurice G. Kendall, *The Advanced Theory of Statistics* (London: C. Griffin & Co., 1943), p. 254.

[2] Martin Wiberg, *The Work Time Distribution* (Chicago: McClure, Hadden, & Ortman, Inc., 1947), p. 4.

[3] W. A. Shewhart, *Economic Control of the Quality of Manufactured Product* (New York: D. Van Nostrand Co., 1931), p. 95.

The principal weakness of the use of the Tchebycheff criterion is that we are inferring a population variance from a sample variance. In a sense, to do this we must have some knowledge of the parent population. Nevertheless, it still gives us a more conservative estimate than we would get if we assumed normalcy. Similar considerations must be extended to the Camp-Meidell revision of the Tchebycheff inequality.

Before listing Tchebycheff's criterion, however, we will develop a standard nomenclature so that our statistical terminology will be consistent:

$\overline{X}_s$ = Sample arithmetic mean
X_{ij} = Any observation i from sample j
θ_s = Any sample statistic at all
θ_p = Any population statistic
$\overline{X}_p$ = Population arithmetic mean
σ_{kj} = Estimated population standard deviation from a sample mean (squared, it is variance instead of standard deviation) of X_{ij} about $\overline{X}_s$, where j is fixed but $i = (1, 2, \ldots . n)$
$\overline{\sigma}_p$ = Mean estimated population standard deviation from a number of samples of X_{ij} about $\overline{X}_p$, where $i = (1, 2, \ldots . n)$ and $j = (1, 2, \ldots . N)$

$$\overline{\sigma}_p = \sum_{j=1} \frac{\sigma_{pj}}{N}$$ provided samples are same size[4]

n = Number of observations in a sample
N = Number of samples in a population
M = Number of observations in a population
$\sigma_{\overline{x}_p}$ = Standard deviation of $\overline{X}_s$ about $\overline{X}_p$

$$\left[\sigma_{\overline{x}_p} = \frac{\overline{\sigma}_p}{\sqrt{n}}\right]$$ (see footnote 5)

σ_{σ_p} = Standard deviation of σ_s about $\overline{\sigma}_p$ (for normal distributions) only where C_2 is a function of sample size $\sigma_{\sigma_p} = \frac{\overline{\sigma}_p}{C_2\sqrt{2n}}$ (see footnote 6)

In examining Tchebycheff's criterion, it is important to recall two requirements:

1. That if a technique is to be valuable for ratesetting purposes in a collective bargaining environment, its range of accuracy is conditioned by the usual increment or decrement to the hourly rate at any one period.

Author's Note on Standard Deviations and Variances. Some texts, Charles C. Peters and Walter R. Voorhis, *Statistical Procedures and Their Mathematical*

[4] *A.S.T.M. Manual on Presentation of Data* (Philadelphia: American Society for Testing Materials, 1941), p. 53.
[5] Shewhart, *op. cit.*, p. 185.
[6] *Ibid.*

Bases (New York: McGraw-Hill Book Co., 1940), p. 69; Paul R. Rider, *An Introduction to Modern Statistical Methods* (New York: John Wiley & Sons, 1939), p. 18, define a sample standard deviation thus:

$$\sigma_s = \sqrt{\frac{\Sigma (X_i - \overline{X}_s)^2}{n}}$$

Then of course $$\sigma_p = \sigma_s \sqrt{\frac{n}{n-1}}$$

A much more rational approach would be to eliminate the concept of a σ_s completely and define

$$\sigma_p = \sqrt{\frac{\Sigma (X_i - \overline{X}_s)^2}{n-1}}$$

The irrational results to which keeping σ_s leads is illustrated in Shewhart. On page 212 of the *Economic Control,* Shewhart defines the sample variance in terms of the population variance:

Thus $$\sigma_s^2 = \sigma_p^2 \frac{M-1}{M}$$

Defining a sample variance in terms of a population variance is putting the cart before the horse. Observations begin as sample collections from the hypothetical population. Thus one would seldom, if ever, define sample variance as a function of the population variance.

Rather generously we have set this figure at plus or minus 5 per cent.

2. The range must be set in such a way that at least 95 per cent of all cases will fall within the range limits.

The first condition will largely be determined by the coefficient of variation multiplied by whatever figure will be required to keep 95 per cent of all cases within the symmetrical range.

Now Tchebycheff's criterion may be stated as follows:[7]

$$P\, t\sigma_\theta > 1 - \frac{1}{t^2}$$

where t is any preassigned number and P is Probability. Thus if we wish to set a probability level of at least 5 per cent to include 95 per cent of all future occurrences, we must assign t the number 5. Thus

$$P\, t\sigma_\theta > 1 - \frac{1}{25} = 96.00$$

Thus, if for $t = 5$, $\frac{5\sigma_{\overline{x}_p}}{\overline{X}_p}$ < 5 per cent over a daily period, the range is acceptable and we have specified a means for deriving a typical standard with a specified accuracy under, however, highly artificial conditions.

A further search for means of inferring population statistics from sample statistics discloses some pertinent observations by the American Society for Testing Materials. They noted that in all the quality control data they had collected at least 96 per cent of all the observations made

[7] *Ibid.*, p. 95.

under statistically controlled conditions fell into symmetrical ranges around the arithmetic mean of plus or minus three standard deviations. This, of course, was made on the basis of minimum numbers of observations of at least 100, a number much larger than is generally used in industrial rate setting.

The probable reason for this is that the Camp-Meidell revision of the Tchebycheff inequality is applicable.[8] Camp and Meidell restricted the parent universe to two conditions: (1) the distribution had to be smooth and uni-modal; (2) the arithmetic mean had to coincide with the mode. Although there is no doubt that Condition 1 is satisfied in rate-setting phenomena, Condition 2 is open to question. However, let us assume that Wiberg's curves, the only ones available, are true; then the difference between the mode and mean may be neglected and the Camp-Meidell inequality applied:

$$P\, t\sigma_\theta \gtrless 1 - \frac{1}{2.25t^2}$$

Thus, when $t = 3$, $1 - \frac{1}{2.25t^2} = .049$

and $P = .951$

Thus $t = 3$ gives us virtually the same probability level for the Camp-Meidell inequality as the Tchebycheff inequality yields for $t = 5$. Hence, if $t = 3$ and $3 \times \frac{\sigma\overline{x}_p}{\overline{X}_p} < 5$ per cent over a daily period, we have reduced the requirements for our determining conditions.

As is well known, if we add to these conditions the requirement of complete normality, then three times the standard deviation sets our range at a probability level of 99.7 per cent of all future readings. Now quite fortunately the distribution of the arithmetic mean of individual samples under a state of statistical control around the population mean assumes a near normal distribution irrespective of the shape of the parent universe. In setting up our range of accuracy, it is important that hourly earnings remain a constant. Hence, if we think in terms of the distribution of hour-long sample arithmetic mean job times around a population arithmetic mean job time of a single day's duration, the three-sigma range will in all likelihood bring our statistically controlled man-machine system within the 99.7 per cent probability limits.

A review of the information we are seeking indicates that it would be of some interest if we were able to infer the skewness and flat-toppedness of the parent population from sample measures. Unfortunately, the measures of these properties that have been worked out are confined to normal parent universes. In addition, the American Society for Testing Materials has observed that the k statistic or measure of skewness has no significance

[8] Leslie E. Simon, *An Engineer's Manual of Statistical Methods* (New York: John Wiley & Sons, 1941), p. 97.

for sample sizes of less than 250.[9] Under the circumstances, we have no alternative but to abandon the search.

It will be recalled that in setting up representative production time values for our idealized factory postulated in Chapter 4, we are assuming that the job time variations make up a statistical population. Furthermore, in creating a system of time study along the lines of statistical quality control, we wish to define it in terms of both the physical cause system and the quantitative characteristics of the sequence of observations produced by such a cause system, chance or constant. We now proceed to an examination of a statistical means of detecting the presence of possible assignable causes of variation that disturb the physical constant cause system. It should be quite clear that the fact that the test indicates no presence of assignable causes is inconclusive. It is a necessary condition but not a sufficient condition; it is merely a check. The test calls for the application of criterion one from the field of quality control. The test is a check to determine whether or not the data we have collected are independent of any time order. I am not here discussing long-run effects that are treated in discussions of the daily work curve and their significance in fatigue. This topic is discussed in Chapter 7 where we treat physiological effects upon the constant chance cause system. This test will check the consistency of the speed and the uniformity of the method used.

The test consists of taking a population of job times, determining the arithmetic mean and population standard deviations, then breaking up the observation into subgroups of four or five observations ordered in time, determining the sample arithmetic means and sample standard deviations.

Control charts are then drawn with the population arithmetic mean as the center line (Fig. 17, p. 282). Since we can assume a normal distribution of the sample means around the population arithmetic mean, control limits on the diagram are drawn parallel to the center line plus and minus $\frac{3\bar{\sigma}_p}{C_2\sqrt{n}}$ away. Thus any sample mean that falls outside these control limits would indicate a possible source of major variation in method or of other origin which has gone undetected but is distorting our constant chance cause system.

Similarly, another chart is drawn for the standard deviation. The center line is made up in this case of the expected population variance determined from all the samples. The control limits are set plus and minus $\frac{3\bar{\sigma}_p}{C_2\sqrt{2n}}$. Any sample variance falling outside these limits is a signal that an assignable cause of variation is present. Finally, it should be emphasized again and again that if no value falls outside the specified limits, it does not necessarily follow that our chance cause system is constant. This criterion is a necessary condition for homogeneity. It is not a sufficient condition.

[9] *A.S.T.M. Manual on Presentation of Data,* p. 26.

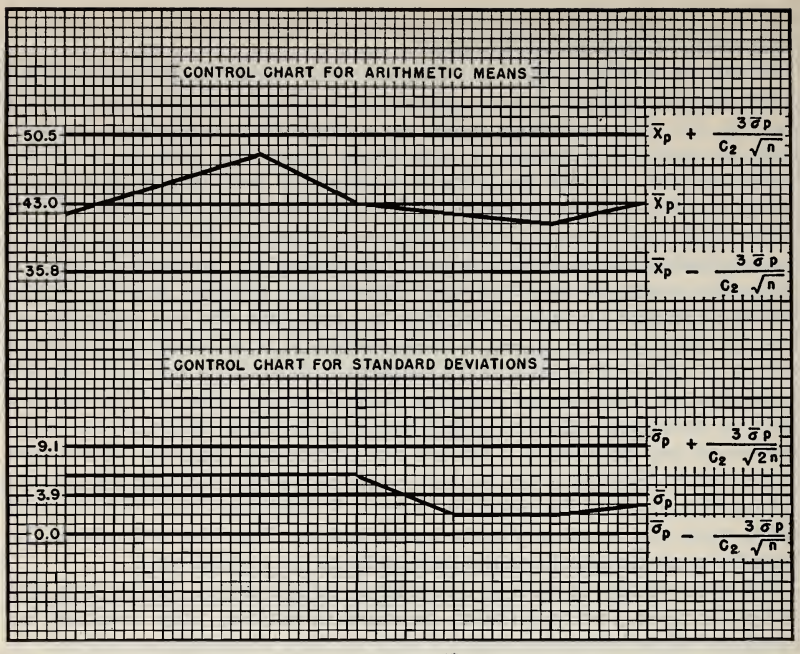


FIGURE 17*

* Martin Wiberg, *The Work Time Distribution* (Chicago: McClure, Hadden & Ortman, Inc., 1947), reproduced by permission of the author.

An illustrative problem is given below. The observation comes from an actual time study taken by a management engineering staff member at a midwestern plant. The rate was in dispute and the union's engineer was trying to determine whether or not the operation was ready for a time study. The test was designed to determine:

1. Whether or not an adequate number of readings were taken in the individual time study;

2. Whether or not the operator had attained that consistency of skill and uniformity of performance which would indicate that she was ready to be studied, assuming all other factors were under control.

In an effort to find an answer to these questions, the total time study was assumed to be a population. This population in turn was stratified into samples of four readings each, as shown in Table X. X_p, the population mean, was computed.

Thus
$$\overline{X}_p = \sum_{i=1}^{28} \frac{X_{ij}}{M} \quad (j = 1, 2, \ldots . 7)$$

where X_{ij} = an individual reading i in subsample j
M = total number of readings in the population

$$\overline{X}_p = 43.1$$

The estimated standard deviation of the parent universe from the population was then computed.

Thus
$$\bar{\sigma}_p^2 = \frac{\Sigma (X_{ij} - \overline{X}_p)^2}{M - 1} \quad \begin{matrix} (i = 1, 2, \ldots . 28) \\ (j = 1, 2, \ldots . 7) \end{matrix}$$
$$\bar{\sigma}_p = 4.27$$

Then
$$\frac{\frac{3\bar{\sigma}_p}{\sqrt{n}}}{\overline{X}_p} \times 100\% = \frac{\frac{3(4.27)}{5.29}}{43.1}$$
$$= 5.64\%$$

where $n = 28$.

Inasmuch as this indicates that there are 997 chances out of 1,000 that any future performance run of 12.07 minutes will not average a $\frac{\frac{3\bar{\sigma}_p}{\sqrt{n}}}{\overline{X}_p}$ $> 5.64\%$, we may conclude that the number of readings taken for the time study are not quite adequate to yield the criterion of plus or minus 5 per cent which we have set. Under the circumstances, another time study should be taken with a larger number of readings.

Next we proceed to examine the consistency of the data by setting up control charts for the arithmetic means and standard deviation of the rational sub-samples.

The central line for the mean control chart is $\overline{\overline{X}}_p = 43.1$.

The limits are $43.1 \pm 3\left(\frac{\bar{\sigma}_p}{C_2\sqrt{n}}\right)$ (see footnote 10).

Thus
$$43.1 \pm 3\left[\frac{3.91}{(.798)\,(\sqrt{4})}\right]$$
$$43.1 \pm 7.35 = \begin{Bmatrix} 50.45 \\ 35.75 \end{Bmatrix}$$

The central line for the standard deviation is 3.91.

The control limits $3.91 \pm 3\left(\frac{\bar{\sigma}_p}{C_2\sqrt{2n}}\right)$

$$3.91 \pm 3\left[\frac{3.91}{(.798)\,(\sqrt{8})}\right]$$

$$3.91 \pm 3\left[\frac{3.91}{(.798)\,(2.83)}\right]$$

$$3.91 \pm 5.19 = \begin{Bmatrix} 9.10 \\ 0.00 \end{Bmatrix}$$

The charting of the sample arithmetic means and standard deviation indicates that the operator possesses the skill and uniformity of method

[10] For C_2 for a sample of size 4, see Shewhart, *op. cit.*, Table 29, p. 185.

TABLE X

j	i	X_{ij}	$X_{ij}-\bar{X}_p$	$X_{ij}-\bar{X}_s$	$(X_{ij}-\bar{X}_p)^2$	$(X_{ij}-\bar{X}_s)^2$
	1	37	6.1	4.5	37.21	20.25
	2	50	−6.9	−8.5	46.61	72.25
	3	42	1.1	− .5	1.21	.25
	4	37	6.1	4.5	37.21	20.25
1		166				$\Sigma = 113.00$
		$\sum_{i=1}^{4} \frac{X_{i1}}{n=4}$ 41.5				$\sigma^2_{p1} = \frac{\Sigma}{n-1}$
						$\sigma^2_{p1} = 37.67$
						$\sigma_{p1} = 6.13$
	5	62	−8.9	−7.2	79.21	51.84
	6	42	1.1	2.8	1.20	7.84
	7	41	2.1	3.8	4.41	14.44
	8	44	− .9	.8	.81	.64
2		179				$\Sigma = 74.76$
		$\sum_{i=5}^{8} \frac{X_{i2}}{n=4}$ 44.8				$\sigma^2_{p2} = \frac{\Sigma}{n-1}$
						$\sigma^2_{p2} = 24.92$
						$\sigma_{p2} = 4.99$
	9	42	1.1	5.3	1.21	28.09
	10	53	−9.9	−5.7	98.01	32.49
	11	47	−3.9	.3	15.21	.90
	12	47	−3.9	.3	15.21	.90
3		189				$\Sigma = 62.38$
		$\sum_{i=9}^{12} \frac{X_{i3}}{n=4}$ 47.3				$\sigma^2_{p3} = \frac{\Sigma}{n-1}$
						$\sigma^2_{p3} = 20.76$
						$\sigma_{p3} = 4.55$
	13	39	4.1	4.3	16.81	18.49
	14	40	3.1	3.3	9.61	10.89
	15	49	−5.9	5.7	34.81	32.49
	16	45	−1.9	1.7	3.61	2.89
4		173				$\Sigma = 64.76$
		$\sum_{i=13}^{16} \frac{X_{i4}}{n=4}$ 43.3				$\sigma^2_{p4} = \frac{\Sigma}{n-1}$
						$\sigma^2_{p4} = 21.59$
						$\sigma_{p4} = 4.64$
	17	39	4.1	3	16.81	9
	18	−44	− .9	−2	.81	4
	19	39	4.1	3	16.81	9
	20	46	−2.9	−4	8.41	16
5		168				$\Sigma = 38$
		$\sum_{i=17}^{20} \frac{X_{i5}}{n=4}$ 42				$\sigma^2_{p5} = \frac{\Sigma}{n-1}$
						$\sigma^2_{p5} = 12.7$
						$\sigma^2_{p5} = 3.39$

TABLE X (continued)

j	i	X_{ij}	$X_{ij}-\bar{X}_p$	$X_{ij}-\bar{X}_s$	$(X_{ij}-\bar{X}_p)^2$	$(X_{ij}-\bar{X}_s)^2$
	21	43	.1	−2.5	.10	6.25
	22	39	4.1	1.5	16.81	2.25
	23	39	4.1	1.5	16.81	2.25
	24	41	2.1	− .5	4.41	.25
6		162				$\Sigma =$ 11.00
		$\sum_{i=21}^{24} \frac{X_{i6}}{n=4}$ 40.5				$\sigma^2_{p6} = \frac{\Sigma}{n-1}$
						$\sigma^2_{p6} =$ 3.67
						$\sigma_{p6} =$ 1.92
	25	42	1.1	.5	1.21	.25
	26	42	1.1	.5	1.21	.25
	27	41	2.1	1.5	4.41	2.25
	28	45	−1.9	−2.5	3.61	6.24
7		170				$\Sigma =$ 9.00
		$\sum_{i=25}^{28} \frac{X_{i7}}{n=4}$ 42.5				$\sigma^2_{p7} = \frac{\Sigma}{n-1}$
						$\sigma^2_{p7} =$ 3.00
						$\sigma_{p7} =$ 1.73

(All readings in hundredths of a minute)

SUMMARY FORMULAE

$\bar{X}_p = \sum_{i=1}^{28} \frac{X_{ij}}{M}$	$\sigma^2_p = \sum_{i=1}^{28} \frac{(X_{ij}-\bar{X}_p)^2}{M-1}$	$\bar{\sigma}_p = \sum_{j=i}^{7} \frac{\sigma_{pj}}{N}$
$\bar{X}_p = \frac{1207}{28}$	$\sigma^2_p = \frac{493.76}{27}$	$\bar{\sigma}_p = \frac{27.35}{7}$
$\bar{X}_p = 43.1$	$\sigma^2_p = 18.29$	$\bar{\sigma}_p = 3.91$
	$\sigma_p = 4.27$	

that would justify taking a time study by assuming a possible state of statistical control. The reader is reminded again that the treament of the distribution of the standard deviations presupposes a normal parent population. This requirement does not extend to the treatment of the arithmetic means.

The failure of the homogeneity test would be a signal to look for certain sources of variation. The test represents a skill criterion for a worker. It indicates rhythm of performance and use of a uniform method. Whether or not a worker should be expected to use one or varied methods is another matter which was discussed in an examination of the whole chance cause system underlying time study practice. Certainly we do know that if

it is to be a predictable phenomenon, we must assume that such is the case. No operator even if he were stalling, and we have excluded stallers by postulating an ideal industrial situation, can work rhythmically unless he has developed skill. This is really a tautology since we have already defined skill in terms of rhythm.

Again, another assignable cause of variation may be the skill of the time study observer himself or the very nature of the technique that calls for elemental breakdowns of total jobs. It may represent a failure by the time study technician to designate consistently a fixed end point for the element.

This does not exhaust all of the possibilities, since we have assumed an infinite chance cause system. Although we are thinking in terms of an idealized shop, it should be clear that this technique offers a tool to the regular practitioner to test whether or not a man-job is ready for a time study such as illustrated above. Many practitioners pride themselves on their intuitive ability to recognize this readiness. The technique here offered may be troublesome, but it is objective.

The method of arriving at the correct number of readings for the time study follows:

Inasmuch as $\sigma_{\bar{x}_p} = \frac{3\sigma_p}{\sqrt{n}}$, n must be determined so that $\frac{3\sigma_{\bar{x}_p}}{\bar{X}_p}$ times 100 is less than 5 per cent. Thus n is determined largely by σ_p and $\bar{X}_p$, the latter two over which we have no control.

Thus the coefficient of variation will serve as an excellent guide in determining what is the optimum size of a time study job element. If the breakdown becomes so minute that the relationship between $\sigma_{\bar{x}_p}$ and $\bar{X}_p$ becomes too close, it is obvious that it will be impossible for $\frac{\sigma_{\bar{x}_p}}{\bar{X}_p}$ to be less than 5 per cent.

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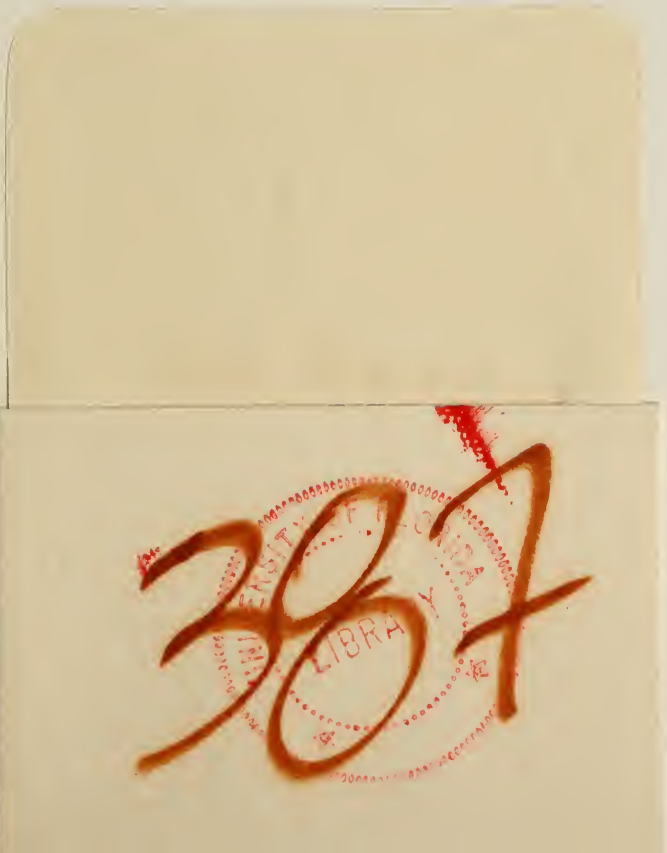
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